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PROOFS AND REFUTATIONS (I)*†

I. LAKATOS

For George Pólya's 75th and Karl Popper's 60th birthday

Introduction

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§ 2. A Proof.

[illegible]icism of the Proof by Counterexamples which are Local but not [illegible]al.

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* Received 3.x.60

† This paper was written in 1958-59 at King's College, Cambridge. It was first read in Karl Popper's seminar in London in March 1959. The paper was then mimeographed and widely circulated. An improved version has been included in the author's Cambridge Ph.D. thesis prepared under Professor R. B. Braithwaite's supervision (*Essays in the Logic of Mathematical Discovery*, 1961). The author also received much help, encouragement and valuable criticism from Dr T. J. Smiley. The thesis would not have been written but for the generous help of the Rockefeller Foundation.

When preparing this latest version at the London School of Economics the author tried to take note especially of the criticisms and suggestions of Dr J. Agassi, Dr I. Hacking, Professors W. C. Kneale and R. Montague, A. Musgrave, Professor M. Polányi and J. W. N. Watkins. The treatment of the exception-barring method was improved under the stimulus of the critical remarks of Professors G. Pólya and B. L. van der Waerden. The distinction between the methods of monster-barring and monster-adjustment was suggested by B. MacLennan.

The paper should be seen against the background of Pólya's revival of mathematical heuristic, and of Popper's critical philosophy.

§ 5. Criticism of the Proof by Counterexamples which are Global but not Local.
  (*a*) The role of refutations in proof-elaboration and theorem-formation. The alliance of proof and refutations.
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§ 6. Concept-formation.

Introduction

It frequently happens in the history of thought that when a powerf new method emerges the study of those problems which can be d with by the new method advances rapidly and attracts the li while the rest tends to be ignored or even forgotten, its stud

This situation seems to have arisen in our century in the Phil of Mathematics as a result of the dynamic development of [illegible] mathematics.

The subject matter of metamathematics is an abstraction of mathematics in which mathematical theories are replaced by formal systems, proofs by certain sequences of well-formed formulae, definitions by 'abbreviatory devices' which are 'theoretically dispensable' but 'typographically convenient'.[1] This abstraction was devised by Hilbert to provide a powerful technique for approaching some of the problems of the methodology of mathematics. At the same time there are problems which fall outside the range of metamathematical abstractions. Among these are all problems relating to informal (*inhaltliche*) mathematics and to its growth, and all problems relating to the situational logic of mathematical problem-solving.

I shall refer to the school of mathematical philosophy which tends to identify mathematics with its metamathematical abstraction (and the philosophy of mathematics with metamathematics) as the 'formalist' school. One of the clearest statements of the formalist position is to be found in Carnap [1937].[2] Carnap demands that (*a*) 'philosophy is to be replaced by the logic of science . . .', (*b*) 'the logic of science is nothing other than the logical syntax of the language of science . . .',

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[1] Church [1956] I, pp. 76-77. Also cf. Peano [1894], p. 49 and Whitehead-Russell [1910-13], I, p. 12. This is an integral part of the Euclidean programme as formulated by Pascal [1657-58]: cf. Lakatos [1962], p. 158.

[2] For full details of this and similar references see the list of works at the end of Part III of this article.

(*c*) 'metamathematics is the syntax of mathematical language' (pp. xiii and 9). Or: philosophy of mathematics is to be replaced by metamathematics.

Formalism disconnects the history of mathematics from the philosophy of mathematics, since, according to the formalist concept of mathematics, there is no history of mathematics proper. Any formalist would basically agree with Russell's 'romantically' put but seriously meant remark, according to which Boole's *Laws of Thought* (1854) was 'the first book ever written on mathematics'.[1] Formalism denies the status of mathematics to most of what has been commonly understood to be mathematics, and can say nothing about its growth. None of the 'creative' periods and hardly any of the 'critical' periods of mathematical theories would be admitted into the formalist heaven, where mathematical theories dwell like the seraphim, purged of all the impurities of earthly uncertainty. Formalists, though, usually leave [illegible]all back door for fallen angels: if it turns out that for some 'mixtures of mathematics and something else' we can find formal systems 'which include them in a certain sense', then they too may be admitted (Curry [1951], pp. 56-57). On those terms Newton had to wait four centuries until Peano, Russell, and Quine helped him into heaven by formalising the Calculus. Dirac is more fortunate: Schwartz saved his soul during his lifetime. Perhaps we should mention here the paradoxical plight of the metamathematician: by formalist, or even by deductivist, standards, he is not an honest mathematician. Dieudonné talks about 'the absolute necessity imposed on any mathematician *who cares for intellectual integrity*' [my italics] to present his reasonings in axiomatic form ([1939], p. 225).

Under the present dominance of formalism, one is tempted to paraphrase Kant: the history of mathematics, lacking the guidance of philosophy, has become *blind*, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become *empty*.

'Formalism' is a bulwark of logical positivist philosophy. According to logical positivism, a statement is meaningful only if it is 'tautological' or empirical. Since informal mathematics is neither

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[1] B. Russell [1901]. The essay was republished as Chapter V of Russell's [1918], under the title 'Mathematics and the Metaphysicians'. In the 1953 Penguin Edition the quotation can be found on p. 74. In the preface of [1918] Russell says of the essay: 'Its tone is partly explained by the fact that the editor begged me to make the article "as romantic as possible".'

'tautological' nor empirical, it must be meaningless, sheer nonsense.[1] The dogmas of logical positivism have been detrimental to the *history and philosophy of mathematics*.

The purpose of these essays is to approach some problems of the *methodology of mathematics*. I use the word 'methodology' in a sense akin to Pólya's and Bernays' 'heuristic'[2] and Popper's 'logic of discovery' or 'situational logic'.[3] The recent expropriation of the term 'methodology of mathematics' to serve as a synonym for 'metamathematics' has undoubtedly a formalist touch. It indicates that in formalist philosophy of mathematics there is no proper place for methodology qua logic of discovery.[4] According to formalists, mathematics is identical with formalised mathematics. But what can one *discover* in a formalised theory? Two sorts of things. *First*, one can discover the solution to problems which a suitably programmed Turing machine could solve in a finite time (such as: is a certain alleged proof a proof or not?). No mathematician is interested in following out the dreary mechanical 'method' prescribed by such decision procedures. *Secondly*, one can discover the solutions to problems (such as: is a certain formula in a non-decidable theory a theorem or not?), where one can be guided only by the 'method' of 'unregimented insight and good fortune'.

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[1] According to Turquette, Gödelian sentences are meaningless ([1950], p. 129). Turquette argues against Copi who claims that since they are *a priori truths* but not analytic, they refute the analytic theory of *a priori* ([1949] and [1950]). Neither of them notices that the peculiar status of Gödelian sentences from this point of [illegible] that these theorems are theorems of informal mathematics, and that [illegible] discuss the status of informal mathematics in a particular case. Neither [illegible] otice[illegible] that theorems of informal mathematics are surely guesses, which [illegible] ardly classify dogmatist-wise as '*a priori*' and '*a posteriori*' guesses.

[2] Pólya [1945], especially p. 102, and also [1954], [1962*a*]; Bernays [illegible] esp. p. 187

[3] Popper [1934], then [1945], especially p. 90 (or the fourth edition (1962 [illegible]. 97); and also [1957], pp. 147 ff.

[4] One can illustrate this, e.g. by Tarski [1930*a*] and Tarski [1930*b*]. In the first paper Tarski uses the term 'deductive sciences' *explicitly* as a shorthand for 'formalised deductive sciences'. He says: 'Formalised deductive disciplines form the field of research of metamathematics roughly in the same sense in which spatial entities form the field of research in geometry.' This sensible formulation is given an intriguing imperialist twist in the second paper: 'The deductive disciplines constitute the subject-matter of the methodology of the deductive sciences in much the same sense in which spatial entities constitute the subject-matter of geometry and animals that of zoology. Naturally not all deductive disciplines are presented in a form suitable for objects of scientific investigation. Those, for example, are not suitable which do not rest on a definite logical basis, have no precise rules of inference, and the theorems of which are formulated in the usually ambiguous and inexact terms of colloquial language—in a word those which are not formalised. Metamathematical investigations are confined in consequence to the discussion of formalised deductive disciplines.' The innovation is that while the first formulation stated that the subject-matter of metamathematics is the formalised deductive disciplines, the second formulation states that the subject-matter of metamathematics is confined to formalised deductive disciplines only because non-formalised deductive sciences are not suitable objects for scientific investigation at all. This implies that the pre-history of a formalised discipline cannot be the subject-matter of a scientific investigation—unlike the pre-history of a zoological species, which can be the subject-matter of a very scientific

Now this bleak alternative between the rationalism of a machine and the irrationalism of blind guessing does not hold for live mathematics:[1] an investigation of *informal* mathematics will yield a rich situational logic for working mathematicians, a situational logic which is neither mechanical nor irrational, but which cannot be recognised still less, stimulated, by the formalist philosophy.

The history of mathematics and the logic of mathematical discovery,

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theory of evolution. Nobody will doubt that some problems about a mathematical theory can only be approached after it has been formalised, just as some problems about human beings (say concerning their anatomy) can only be approached after their death. But few will infer from this that human beings are 'suitable for scientific investigation' only when they are 'presented in "dead" form', and that biological investigations are confined in consequence to the discussion of dead human beings—although, I should not be surprised if some enthusiastic pupil of Vesalius in those glorious days of early anatomy, when the powerful new method of dissection emerged, had identified biology with the analysis of dead bodies.

In the preface of his [1941] Tarski enlarges on his negative attitude towards the possibility of any sort of methodology other than formal systems: 'A course in the methodology of empirical sciences . . . must be largely confined to evaluations and criticisms of tentative gropings and unsuccessful efforts.' The reason is that empirical sciences are unscientific: for Tarski defines a scientific theory 'as a system of asserted statements arranged according to certain rules' (Ibid.).

[1] One of the most dangerous vagaries of formalist philosophy is the habit of (1) stating something—rightly—about formal systems; (2) then saying that this applies to 'mathematics'—this is again right if we accept the identification of mathematics and formal systems; (3) subsequently, with a surreptitious shift in meaning, using the term 'mathematics' in the ordinary sense. So Quine says ([1951], p. 87), that 'this reflects the characteristic mathematical situation: the mathematician hits upon his proof by unregimented insight and good fortune, but afterwards other mathematicians can check his proof'. But often the checking of an *ordinary* proof is a very delicate enterprise, and to hit on a 'mistake' requires as much insight and luck as to hit on a proof: the discovery of 'mistakes' in informal proofs may sometimes take decades—if not centuries.

i.e. the phylogenesis and the ontogenesis of mathematical thought,[1] cannot be developed without the criticism and ultimate rejection of formalism.

But formalist philosophy of mathematics has very deep roots. It is the latest link in the long chain of *dogmatist* philosophies of mathematics. For more than two thousand years there has been an argument between *dogmatists* and *sceptics*. The dogmatists hold that—by the power of our human intellect and/or senses—we can attain truth and know that we have attained it. The sceptics on the other hand either hold that we cannot attain the truth at all (unless with the help of mystical experience), or that we cannot know if we can attain it or that we have attained it. In this great debate, in which arguments are time and again brought up-to-date, mathematics has been the proud fortress of dogmatism. Whenever the mathematical dogmatism of the day got into a 'crisis', a new version once again provided genuine rigour and ultimate foundations, thereby restoring the [illegible] authoritative, infallible, irrefutable mathematics, 'the only [illegible] that it has pleased God hitherto to bestow on mankind' (Hobbes [1651], p. 15). Most sceptics resigned themselves to the impregnability of this stronghold of dogmatist epistemology.[2] A challenge is now overdue.

The core of this case-study will challenge mathematical formalism, but will not challenge directly the ultimate positions of mathematical dogmatism. Its modest aim is to elaborate the point that informal, quasi-empirical, mathematics does not grow through a monotonous increase of the number of indubitably established theorems but through the incessant improvement of guesses by speculation and criticism, by the logic of proofs and refutations. Since however metamathematics is a paradigm of informal, quasi-empirical mathematics just now in rapid growth, the essay, by implication, will also challenge modern mathematical dogmatism. The student of recent history of metamathematics will recognise the patterns described here in his own field.

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[1] Both H. Poincaré and G. Pólya propose to apply E. Haeckel's 'fundamental biogenetic law' about ontogeny recapitulating phylogeny to mental development, in particular to mathematical mental development. (Poincaré [1908], p. 135, and Pólya [1962b].) To quote Poincaré: 'Zoologists maintain that the embryonic development of an animal recapitulates in brief the whole history of its ancestors throughout geologic time. It seems it is the same in the development of minds. . . . For this reason, the history of science should be our first guide' (C. B. Halsted's authorised translation, p. 437).

[2] For a discussion of the rôle of mathematics in the dogmatist-sceptic controversy, cf. my [1962].

The dialogue form should reflect the dialectic of the story; it is meant to contain a sort of *rationally reconstructed or 'distilled' history.* *The real history will chime in in the footnotes, most of which are to be taken, therefore, as an organic part of the essay.*

1 *A Problem and a Conjecture*

The dialogue takes place in an imaginary classroom. The class gets interested in a *PROBLEM*: is there a relation between the number of vertices V, the number of edges E and the number of faces F of polyhedra—particularly of *regular polyhedra*—analogous to the trivial relation between the number of vertices and edges of *polygons*, namely, that there are as many edges as vertices: $V=E$? This latter relation enables us to classify *polygons* according to the number of edges (or vertices): triangles, quadrangles, pentagons, etc. An analogous relation would help to classify *polyhedra.*

[illegible] much trial and error they notice that for all regular polyhedra [illegible] $E + F = 2$.[1] Somebody *guesses* that this may apply for any polyhedron whatsoever. Others try to falsify this *conjecture*, try to test it in many different ways—it holds good. The results *corroborate* the conjecture, and suggest that it could be *proved*. It is at this point—after the stages *problem* and *conjecture*—that we enter the classroom.[1] The teacher is just going to offer a *proof*.

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[1] First noticed by Euler [1750]. His original problem was the classification of polyhedra, the difficulty of which was pointed out in the editorial summary: 'While in plane geometry polygons (*figurae rectilineae*) could be classified very easily according to the number of their sides, which of course is always equal to the number of their angles, in stereometry the classification of polyhedra (*corpora hedris planis inclusa*) represents a much more difficult problem, since the number of faces alone is insufficient for this purpose.' The key to Euler's result was just the invention of the concepts of *vertex* and *edge*: it was he who first pointed out that besides the number of faces the number of *points* and *lines* on the surface of the polyhedron determines its (topological) character. It is interesting that on the one hand he was eager to stress the novelty of his conceptual framework, and that he had to invent the term '*acies*' (edge) instead of the old '*latus*' (side), since *latus* was a polygonal concept while he wanted a polyhedral one, on the other hand he still retained the term '*angulus solidus*' (solid angle) for his point-like vertices. It has been recently generally accepted that the priority of the result goes to Descartes. The ground for this claim is a manuscript of Descartes [*ca.* 1639] copied by Leibniz in Paris from the original in 1675–6, and rediscovered and published by Foucher de Careil in 1860. The priority should not be granted to Descartes without a minor qualification. It is true that Descartes states that the number of plane angles equals $2\phi + 2\alpha - 4$ where by ϕ he means the number of faces and by α the number of solid angles. It is also true that he states that there are twice as many plane angles as edges (*latera*). The trivial conjunction of these two statements of course yields the Euler formula. But Descartes did not see the point of doing so, since he still thought in terms of angles (plane and solid) and faces, and did not make a conscious revolutionary change to the concepts of 0-dimensional vertices, 1-dimensional edges and 2-dimensional faces as a necessary and sufficient basis for the full topological characterisation of polyhedra.

2. *A Proof*

TEACHER: In our last lesson we arrived at a conjecture concerning polyhedra, namely, that for all polyhedra $V-E+F=2$, where V is the number of vertices, E the number of edges and F the number of faces. We tested it by various methods. But we haven't yet proved it. Has anybody found a proof?

PUPIL SIGMA: 'I for one have to admit that I have not yet been able to devise a strict proof of this theorem. . . . As however the truth of it has been established in so many cases, there can b[illegible] that it holds good for any solid. Thus the proposition se[illegible] satisfactorily demonstrated.'[2] But if you have a proof, please do present it.

TEACHER: In fact I have one. It consists of the following thought-experiment. *Step 1:* Let us imagine the polyhedron to be hollow, with a surface made of thin rubber. If we cut out one of the faces, we can stretch the remaining surface flat on the blackboard, without tearing it. The faces and edges will be deformed, the edges may become curv[illegible] but V, E and F will not alter, so that if and only if $V-E+F=$ [illegible] the original polyhedron, then $V-E+F=1$ for this flat net[illegible] remember that we have removed one face. (Fig. 1 shows [illegible] flat network for the case of a cube.) *Step 2:* Now we triangulate our map—it does indeed look like a geographical map. We draw (possibly curvilinear) diagonals in those (possibly curvilinear) polygons which are not already (possibly curvilinear) triangles. By drawing each diagonal we increase both E and F by one, so that the total $V - E + F$ will not be altered (Fig. 2). *Step 3:* From the triangulated network we now remove the triangles one by one. To remove a triangle we either remove an edge—upon which one face and one edge disappear (Fig. 3*a*), or we remove two edges and a vertex—upon which one face, two edges and one vertex disappear (Fig. 3*b*). Thus if $V - E + F = 1$ before a triangle is removed, it remains so after the triangle is removed. At the end of this procedure we get a single triangle. For this $V - E + F = 1$ holds true. Thus we have proved our conjecture.[1]

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[1] Euler tested the conjecture quite thoroughly for consequences. He checked it for prisms, pyramids and so on. He could have added that the proposition that there are only five regular bodies is also a consequence of the conjecture. Another suspected consequence is the hitherto corroborated proposition that four colours are sufficient to colour a map.

The phase of *conjecturing* and *testing* in the case of $V-E+F=2$ is discussed in Pólya ([1954], Vol. I, the first five sections of the third chapter, pp. 35-41). Pólya stopped here, and does not deal with the phase of *proving*—though of course he points out the need for a heuristic of 'problems to prove' ([1945], p. 144). Our discussion starts where Pólya stops.

[2] Euler ([1750], p. 119 and p. 124). But later [1751] he proposed a proof.

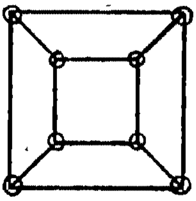

FIG. 1

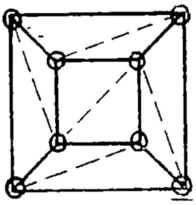

FIG. 2

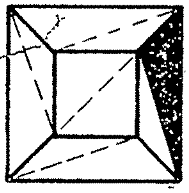

FIG. 3*a*

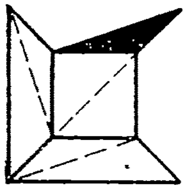

FIG. 3*b*

PUPIL DELTA: You should now call it a *theorem*. There is nothing conjectural about it any more.[2]

PUPIL ALPHA: I wonder. I see that this experiment can be performed for a cube or for a tetrahedron, but how am I to know that it can be performed for *any* polyhedron? For instance, are you sure, Sir, that *any polyhedron, after having a face removed, can be stretched flat on the blackboard*? I am dubious about your first step.

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[1] This proof-idea stems from Cauchy [1811].

[2] Delta's view that this proof has established the 'theorem' beyond doubt was shared by many mathematicians in the nineteenth century, e.g. Crelle [1826–27], II, pp. 668–671, Matthiessen [1863], p. 449, Jonquières [1890*a*] and [1890*b*]. To quote a characteristic passage: 'After Cauchy's proof, it became absolutely indubitable that the elegant relation $V + F = E + 2$ applies to all sorts of polyhedra, just as Euler stated in 1752. In 1811 all indecision should have disappeared.' Jonquières [1890*a*], pp. 111–112.

Pupil Beta: Are you sure that *in triangulating the map one will always get a new face for any new edge*? I am dubious about your second step.

Pupil Gamma: Are you sure that *there are only two alternatives—the disappearance of one edge or else of two edges and a vertex—when one drops the triangles one by one*? Are you even sure that *one is left with a single triangle at the end of this process*? I am dubious about your third step.[1]

Teacher: Of course I am not sure.

Alpha: But then we are worse off than before! Instead of one conjecture we now have at least three! And this you call a 'proof'!

Teacher: I admit that the traditional name 'proof' for this thought-experiment may rightly be considered a bit misleading. I do not think that it establishes the truth of the conjecture.

Delta: What does it do then? What do you think a mathematical proof proves?

Teacher: This is a subtle question which we shall try to answer later. Till then I propose to retain the time-honoured technical term 'proof' for a *thought-experiment—or 'quasi-experiment'—which suggests a decomposition of the original conjecture into subconjectures or lemmas*, thus *embedding it* in a possibly quite distant body of knowledge. Our 'proof', for instance, has embedded the original conjecture—about crystals, or, say, solid[illegible] the theory of rubber sheets. Descartes or Euler, the fathers of the original conjecture, certainly did not even dream of this.[2]

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[1] The class is a rather advanced one. To Cauchy, Poinsot, and to many other excellent mathematicians of the nineteenth century these questions did not occur.

[2] Thought-experiment (*deiknymi*) was the most ancient pattern of mathematical proof. It prevailed in pre-Euclidean Greek mathematics (cf. Á. Szabó [1958]).

That conjectures (or theorems) precede proofs in the heuristic order was a commonplace for ancient mathematicians. This followed from the heuristic precedence of '*analysis*' over '*synthesis*'. (For an excellent discussion *see* Robinson [1936].) According to Proclos, '. . . it is . . . necessary to know beforehand what is sought' (Heath [1925], I, p. 129). 'They said that a theorem is that which is proposed with a view to the demonstration of the very thing proposed'—says Pappus (ibid. I, p. 10). The Greeks did not think much of propositions which they happened to hit upon in the deductive direction without having previously guessed them. They called them *porisms*, corollaries, incidental results springing from the proof of a theorem or the solution of a problem, results not directly sought but appearing, as it were, by chance, without any additional labour, and constituting, as Proclus says, a sort of windfall (*ermaion*) or bonus (*kerdos*) (ibid. I, p. 278). We read in the editorial summary to Euler [1753] that arithmetical theorems 'were discovered

3. *Criticism of the Proof by Counterexamples which are Local but not Global.*

TEACHER: This decomposition of the conjecture suggested by the proof opens new vistas for testing. The decomposition deploys the conjecture on a wider front, so that our criticism has more targets. We now have at least three opportunities for counterexamples instead of one!

GAMMA: I already expressed my dislike of your third lemma (viz. that in removing triangles from the network which resulted from the stretching and subsequent triangulation, we have only two possibilities: either we remove an edge or we remove two edges and a vertex). I suspect that other patterns may emerge when removing a triangle.

TEACHER: Suspicion is not criticism.

GAMMA: Then is a *counterexample* criticism?

TEACHER: Certainly. Conjectures ignore dislike and suspicion, but they cannot ignore counterexamples.

THETA (*aside*): Conjectures are obviously very different from those who represent them.

GAMMA: I propose a trivial counterexample. Take the triangular network which results from performing the first two operations on a cube (Fig. 2). Now if I remove a triangle from the *inside* of this network, as one might take a piece out of a jigsaw puzzle, I remove one triangle without removing a single edge or vertex. So the third lemma

long before their truth has been confirmed by rigid demonstrations'. Both the Editor and Euler use for this process of discovery the modern term '*induction*' instead of the ancient '*analysis*' (ibid.). The heuristic precedence of the result over the argument, of the theorem over the proof, has deep roots in mathematical folklore. Let us quote some variations on a familiar theme: Chrysippus is said to have written to Cleanthes: 'Just send me the theorems, then I shall find the proofs' (cf. Diogenes Laertius [ca. 200], VII. 179). Gauss is said to have complained: 'I have had my results for a long time; but I do not yet know how I am to arrive at them' (cf. Arber [1954], p. 47), and Riemann: 'If only I had the theorems! Then I should find the proofs easily enough.' (Cf. Hölder [1924], p. 487.) Pólya stresses: 'You have to guess a mathematical theorem before you prove it' ([1954], Vol. I, p. vi).

The term '*quasi-experiment*' is from the above-mentioned editorial summary to Euler [1753]. According to the Editor: 'As we must refer the numbers to the pure intellect alone, we can hardly understand how observations and *quasi-experiments* can be of use in investigating the nature of the numbers. Yet, in fact, as I shall show here with very good reasons, the properties of the numbers known today have been mostly discovered by observation . . .' (Pólya's translation; he mistakenly attributes the quotation to Euler in his [1954], I, p. 3).

is false—and not only in the case of the cube, but for *all* polyhedra except the tetrahedron, in the flat network of which all the triangles are boundary triangles. Your proof thus proves the Euler theorem for the tetrahedron. But we already *knew* that $V - E + F = 2$ for the tetrahedron, so why prove it?

TEACHER: You are right. But notice that the cube which is a counterexample to the third lemma is not also a counterexample to the main conjecture, since for the cube $V - E + F = 2$. You have shown the poverty of the argument—the proof—but not the falsity of our conjecture.

ALPHA: Will you scrap your proof then?

TEACHER: No. Criticism is not necessarily destruction. I shall improve my proof so that it will stand up to the criticism.

GAMMA: How?

TEACHER: Before showing how, let me introduce the following terminology. I shall call a '*local counterexample*' an example which refutes a lemma (without necessarily refuting the main conjecture), and I shall call a '*global counterexample*' an example which refutes the main conjecture itself. Thus your counterexample is local but not global. A local, but not global, counterexample is a criticism of the proof, but not of the conjecture.

GAMMA: So, the conjecture may be true, but your proof does not prove it.

TEACHER: But I can easily elaborate, *improve the proof*, by replacing the false lemma by a slightly modified one, which your counterexample will not refute. I no longer contend that *the removal of any triangle follows one of the two patterns mentioned*, but merely that *at each stage of the removing operation the removal of any boundary triangle follows one of these patterns*. Coming back to my thought-experiment, all that I have to do is to insert a single word in my third step, to wit, that 'from the triangulated network we now remove the *boundary* triangles one by one'. You will agree that it only needed a trifling observation to put the proof right.[1]

GAMMA: I do not think your observation was so trifling; in fact it was quite ingenious. To make this clear I shall show that it is false. Take the flat network of the cube again and remove eight of the ten triangles in the order given in Fig. 4. At the removal of the eighth triangle, which is certainly by then a boundary triangle, we removed two edges and no vertex—this changes $V-E+F$ by 1. And we are left with the two disconnected triangles 9 and 10.

---

[1] Lhuilier, when correcting in a similar way a proof of Euler, says that he made only a 'trifling observation' ([1812-13], p. 179). Euler himself, however, gave the proof up, since he noticed the trouble but could not make that 'trifling observation'.

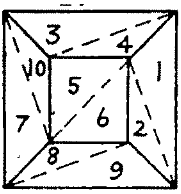

FIG. 4

TEACHER: Well, I might save face by saying that I meant by a boundary triangle a triangle whose removal does not disconnect the network. But intellectual honesty prevents me from making surreptitious changes in my position by sentences starting with 'I meant . . .' so I admit that now I must *replace* the second version of the triangle-removing operation with a third version: that we remove the triangles one by one in such a way that $V-E+F$ does not alter.

KAPPA: I generously agree that the lemma corresponding to this operation is true: namely, that if we remove the triangles one by one in such a way that $V-E+F$ does not alter, then $V-E+F$ does not alter.

TEACHER: No. The lemma is that *the triangles in our network can be so numbered that in removing them in the right order $V-E+F$ will not alter till we reach the last triangle.*

KAPPA: But how should one construct this right order, if it exists at all?[1] Your original thought-experiment gave the instructions: remove the triangles in any order. Your modified thought-experiment gave the instruction: remove boundary triangles in any order. Now you say we should follow a definite order, but you do not say which and whether that order exists at all. Thus the thought-experiment breaks down. You improved the proof-analysis, i.e. the list of lemmas; but the thought-experiment which you called 'the proof' has disappeared.

RHO: Only the third step has disappeared.

---

[1] Cauchy thought that the instruction to find at each stage a triangle which can be removed either by removing two edges and a vertex or one edge can be trivially carried out for any polyhedron ([1811], p. 79). This is of course connected with his inability to imagine a polyhedron that is not homeomorphic with the sphere.

KAPPA: Moreover, did you *improve* the lemma? Your first two simple versions at least looked trivially true before they were refuted; your lengthy, patched up version does not even look plausible. Can you really believe that it will escape refutation?

TEACHER: 'Plausible' or even 'trivially true' propositions are usually soon refuted: sophisticated, implausible conjectures, matured in criticism, might hit on the truth.

OMEGA: And what happens if even your 'sophisticated conjectures' are falsified and if this time you cannot replace them by unfalsified ones? Or, if you do *not* succeed in improving the argument further by local patching? You have succeeded in getting over a local counterexample which was not global by replacing the refuted lemma. What if you do not succeed next time?

TEACHER: Good question—it will be put on the agenda for tomorrow.

4. *Criticism of the Conjecture by Global Counterexamples*

ALPHA: I have a counterexample which will falsify your first lemma—but this will also be a counterexample to the main conjecture, i.e. this will be a global counterexample as well.

TEACHER: Indeed! Interesting. Let us see.

ALPHA: Imagine a solid bounded by a pair of nested cubes—a pair of cubes, one of which is inside, but does not touch the other (Fig. 5).

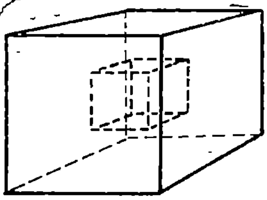

FIG. 5

This hollow cube falsifies your first lemma, because on removing a face from the inner cube, the polyhedron will not be stretchable on to a plane. Nor will it help to remove a face from the outer cube instead. Besides, for each cube $V-E+F=2$, so that for the hollow cube $V-E+F=4$.

TEACHER: Good show. Let us call it *Counterexample 1*.[1] Now what?

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[1] This *Counterexample 1* was first noticed by Lhuilier ([1812–13], p. 194). But Gergonne, the Editor, added (p. 186) that he himself noticed this long before Lhuilier's

(a) *Rejection of the conjecture. The method of surrender*

Gamma: Sir, your composure baffles me. A single counterexample refutes a conjecture as effectively as ten. The conjecture and its proof have completely misfired. Hands up! You have to surrender. Scrap the false conjecture, forget about it and try a radically new approach.

Teacher: I agree with you that the *conjecture* has received a severe criticism by Alpha's counterexample. But it is untrue that the *proof* has 'completely misfired'. If, for the time being, you agree to my earlier proposal to use the word 'proof' for a 'thought-experiment which leads to decomposition of the original conjecture into subconjectures', instead of using it in the sense of a 'guarantee of certain truth', you need not draw this conclusion. My proof certainly proved Euler's conjecture in the first sense, but not necessarily in the second. You are interested only in proofs which 'prove' what they have set out to prove. I am interested in proofs even if they do not accomplish their intended task. Columbus did not reach India but he discovered something quite interesting.

Alpha: So according to your philosophy—while a local counterexample (if it is not global at the same time) is a criticism of the proof, but not of the conjecture—a global counterexample is a criticism of the conjecture, but not necessarily of the proof. You agree to surrender as regards the conjecture, but you defend the proof. But if the conjecture is false, what on earth does the proof prove?

Gamma: Your analogy with Columbus breaks down. Accepting a global counterexample must mean total surrender.

(b) *Rejection of the counterexample. The method of monster-barring*

Delta: But why accept the counterexample? We proved our onjecture—now it is a theorem. I admit that it clashes with this -called 'counterexample'. One of them has to give way. But y should the theorem give way, when it has been proved? It is 'criticism' that should retreat. It is fake criticism. This pair of

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. Not so Cauchy, who published his proof just a year before. And this erexample was to be rediscovered twenty years later by Hessel ([1832], p. 16). Lhuilier and Hessel were led to their discovery by mineralogical collections in which they noticed some double crystals, where the inner crystal is not translucent, but the outer is. Lhuilier acknowledges the stimulus of the crystal collection of his friend Professor Pictet ([1812–13], p. 188). Hessel refers to lead sulphide cubes enclosed in translucent calcium fluoride crystals ([1832], p. 16).

nested cubes is not a polyhedron at all. It is a *monster*, a pathological case, not a counterexample.

GAMMA: Why not? *A polyhedron is a solid whose surface consists of polygonal faces.* And my counterexample is a solid bounded by polygonal faces.

TEACHER: Let us call this definition *Def. 1*.[1]

DELTA: Your definition is incorrect. A polyhedron must be a *surface*: it has faces, edges, vertices, it can be deformed, stretched out on a blackboard, and has nothing to do with the concept of 'solid'. *A polyhedron is a surface consisting of a system of polygons.*

TEACHER: Call this *Def. 2*.[2]

DELTA: So really you showed us *two* polyhedra—*two* surfaces, one completely inside the other. A woman with a child in her womb is not a counterexample to the thesis that human beings have one head.

ALPHA: So! My counterexample has bred a new concept of polyhedron. Or do you dare to assert that by polyhedron you *always* meant a surface?

TEACHER: For the moment let us accept Delta's *Def. 2*. Can you refute our conjecture now if by polyhedron we mean a surface?

ALPHA: Certainly. Take two tetrahedra which have an edge in common (Fig. 6*a*). Or, take two tetrahedra which have a vertex in common (Fig. 6*b*). Both these twins are connected, both constitute one single surface. And, you may check that for both $V - E + F = 3$

TEACHER: *Counterexamples 2a and 2b*.[3]

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[1] *Definition 1* occurs first in the eighteenth century; e.g.: 'One gives the name *polyhedral solid*, or simply *polyhedron*, to any solid bounded by planes or plane faces' (Legendre [1794], p. 160). A similar definition is given by Euler ([1750]). Euclid, while defining cube, octahedron, pyramid, prism, does not define the general term polyhedron, but occasionally uses it (e.g. Book XII, Second Problem, Prop. 17).

[2] We find *Definition 2* implicitly in one of Jonquières' papers read to the Frenc Academy against those who meant to refute Euler's theorem. These papers are thesaurus of monsterbarring techniques. He thunders against Lhuilier's monstro pair of nested cubes: 'Such a system is not really a polyhedron, but a pair of disti polyhedra, each independent of the other. . . . A polyhedron, at least from classical point of view, deserves the name only if, before all else, a point can n continuously over its entire surface; here this is not the case . . . This first exce of Lhuilier can therefore be discarded' ([1890*b*], p. 170). This definition—as op to Definition 1—goes down very well with analytical topologists who are not i ested at all in the theory of polyhedra as such but as a handmaiden for the theory of surfaces.

[3] *Counterexamples 2a* and *2b* were missed by Lhuilier and first discovered only by Hessel ([1832], p. 13).

DELTA: I admire your perverted imagination, but of course I did not mean that *any* system of polygons is a polyhedron. By polyhedron I meant *a system of polygons arranged in such a way that (1) exactly two polygons meet at every edge and (2) it is possible to get from the inside of any polygon to the inside of any other polygon by a route which never crosses any edge at a vertex.* Your first twins will be excluded by the first criterion in my definition, your second twins by the second criterion.

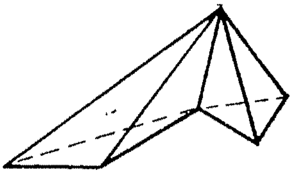


FIG. 6*a*

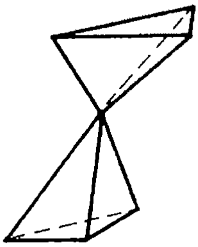


FIG. 6*b*

TEACHER: *Def. 3.*[1]

ALPHA: I admire your perverted ingenuity in inventing one definition after another as barricades against the falsification of your pet ideas. Why don't you just define a polyhedron as a system of polygons for which the equation $V - E + F = 2$ holds, and this Perfect Definition. . . .

KAPPA: *Def. P.*[2]

ALPHA: . . . would settle the dispute for ever? There would be no need to investigate the subject any further.

DELTA: But there isn't a theorem in the world which couldn't be falsified by monsters.

---

[1] *Definition 3* first turns up to keep out twintetrahedra in Möbius ([1865], p. 32). We find his cumbersome definition reproduced in some modern textbooks in the sual authoritarian 'take it or leave it' way; the story of its monsterbarring back- ound—that would at least explain it—is not told (e.g. Hilbert-Cohn Vossen [1956], 290).

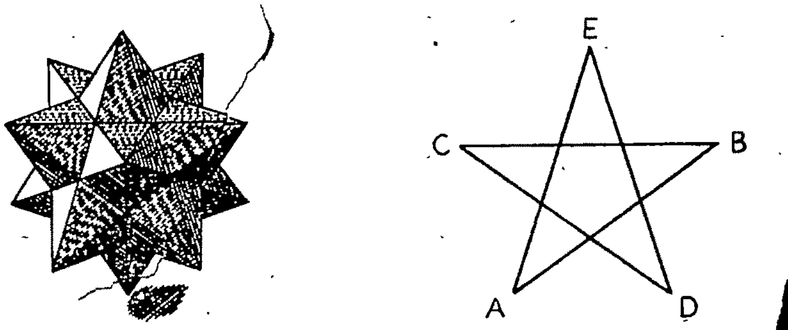
[2] *Definition P* according to which Eulerianness would be a definitional character- of polyhedra was in fact suggested by R. Baltzer: 'Ordinary polyhedra are sionally (following Hessel) called Eulerian polyhedra. It would be more ppriate to find a special name for non-genuine (*uneigentliche*) polyhedra' ([1860], II, p. 207). The reference to Hessel is unfair: Hessel used the term 'Eulerian' simply as an abbreviation for polyhedra for which Euler's relation holds in contradistinction to the non-Eulerian ones ([1832], p. 19). For *Def. P* see also the Schläfli quotation in footnote pp. 18–19.

TEACHER: I am sorry to interrupt you. As we have seen, refutation by counterexamples depends on the meaning of the terms in question. If a counterexample is to be an objective criticism, we have to agree on the meaning of our terms. We *may* achieve such an agreement by defining the term where communication broke down. I, for one, didn't define 'polyhedron'. I assumed *familiarity* with the concept, i.e. the ability to distinguish a thing which is a polyhedron from a thing which is not a polyhedron—what some logicians call knowing the extension of the concept of polyhedron. It turned out that the extension of the concept wasn't at all obvious: *definitions are frequently proposed and argued about when counterexamples emerge.* I suggest that we now consider the rival definitions together, and leave until later the discussion of the differences in the results which will follow from choosing different definitions. Can anybody offer something which even the most restrictive definition would allow as a real counterexample?

KAPPA: Including *Def. P*?

TEACHER: Excluding *Def. P*.

GAMMA: I can. Look at this *Counterexample 3*: a star-polyhedron —I shall call it *urchin* (Fig. 7). This consists of 12 star-pentagons (Fig. 8). It has 12 vertices, 30 edges, and 12 pentagonal faces—you may check it if you like by counting. Thus the Descartes-Euler th is not true at all, since for this polyhedron $V - E + F = -6$.[1]



FIGS. 7 and 8. Kepler (Fig. 7) shaded each face in a different way to show which triangles belong to the same pentagonal face.

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[1] The 'urchin' was first discussed by Kepler in his cosmological theory ([1 *Lib.* II, XIX and XXVI, on p. 52 and p. 60 and *Lib.* V, *Cap.* I, p. 182, *Cap.* III, p. 187 and *Cap.* IX, XLVII). The name 'urchin' is Kepler's ('*cui nomen Echino feci*'). Fig. 7 is copied from his book (p. 52) which contains also another picture on p. 182.

DELTA: Why do you think that your 'urchin' is a polyhedron?

GAMMA: Do you not see? This is a polyhedron, whose faces are the twelve star-pentagons. It satisfies your last definition: it is 'a system of polygons arranged in such a way that (1) exactly two polygons meet at every edge, and (2) it is possible to get from every polygon to every other polygon without ever crossing a vertex of the polyhedron'.

DELTA: But then you do not even know what a polygon is! A star-pentagon is certainly not a polygon! *A polygon is a system of edges arranged in such a way that (1) exactly two edges meet at every vertex, and (2) the edges have no points in common except the vertices.*

TEACHER: Let us call this *Def. 4*.

GAMMA: I don't see why you include the second clause. The right definition of the polygon should contain the first clause only.

TEACHER: *Def. 4'*.

GAMMA: The second clause has nothing to do with the essence of a polygon. Look: if I lift an edge a little, the star-pentagon is already a polygon even in your sense. You imagine a polygon to be drawn in chalk on the blackboard, but you should imagine it as a wooden structure: then it is clear that what you think to be a point in common is not really one point, but two different points lying one above the other. You are misled by your embedding the polygon in a plane—you should let its limbs stretch out in space![1]

Poinsot independently rediscovered it, and it was he who pointed out that the Euler formula did not apply to it ([1809], p. 48). The now standard term 'small stellated polyhedron' is Cayley's ([1859], p. 125). Schläfli admitted star-polyhedra in general, but nevertheless rejected our small stellated dodecahedron as a monster. According to him 'this is not a genuine polyhedron, for it does not satisfy the condition $V - E + F = 2$' ([1852], § 34).

[1] The dispute whether polygon should be defined so as to include star-polygons not (*Def. 4* or *Def. 4'*) is a very old one. The argument put forward in our dialogue isuc star-polygons can be embedded as ordinary polygons in a space of higher occa ns—is a modern topological argument, but one can put forward many others. appr sot defending his star-polyhedra argued for the admission of star-polygons Vol. uments taken from analytical geometry: '. . . all these distinctions (between "ordinary" and "star"-polygons) are more apparent than real, and they completely disappear in the analytical treatment, in which the various species of polygons are quite inseparable. To the edge of a regular polygon there corresponds an equation with real roots, which simultaneously yields the edges of all the regular polygons of the same order. Thus it is not possible to obtain the edges of a regular inscribed heptagon, without at the same time finding edges of heptagons of the second and third species. Conversely, given the edge of a regular heptagon, one may determine the

DELTA: Would you mind telling me what is the *area* of a star-pentagon? Or would you say that some polygons have no area?

GAMMA: Was it not you yourself who said that a polyhedron has nothing to do with the idea of solidity? Why now suggest that the idea of polygon should be linked with the idea of area? We agreed that a polyhedron is a closed surface with edges and vertices—then why not agree that a polygon is simply a closed curve with vertices? But if you stick to your idea I am willing to define the area of a star-polygon.[1]

TEACHER: Let us leave this dispute for a moment, and proceed as before. Consider the last two definitions together—*Def. 4* and *Def. 4′*. Can anyone give a counterexample to our conjecture that will comply with *both* definitions of polygons?

ALPHA: Here is one. Consider a *picture-frame* like this (Fig. 9). This is a polyhedron according to any of the definitions hitherto proposed. Nonetheless you will find, on counting the vertices, edges and faces, that $V - E + F = 0$.

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radius of a circle in which it can be inscribed, but in so doing, one will find three different circles corresponding to the three species of heptagon which may be constructed on the given edge; similarly for other polygons. Thus we are justified in giving the name " polygon " to these new starred figures ' ([1809], p. 26). Schröder uses the Hankelian argument: ' The extension to rational fractions of the power concept originally associated only with the integers has been very fruitful in Algebra; this suggests that we try to do the same thing in geometry whenever the opportunity presents itself . . . ' ([1862], p. 56). Then he shows that we may find a geometrical interpretation for the concept of p/q-sided polygons in the star-polygons.

[1] Gamma's claim that he can define the area for star-polygons is not a bluff. Some of those who defended the wider concept of polygon solved the problem by putting forward a wider concept of the area of polygon. There is an especially obvious way to do this in the case of regular star-polygons. We may take the area of a polygon as the sum of the areas of the isosceles triangles which join the centre of the inscribed or circumscribed circle to the sides. In this case, of course, some ' portions ' of the star-polygon will count more than once. In the case of irregular polygons where we have not got any one distinguished point, we may still take any point as origin and treat negatively oriented triangles as having negative areas (Meister [1769-70], p. 179). It turns out—and this can certainly be expected from an ' area '—that the area thus defined will not depend on the choice of the origin (Möbius [1827], p. 218). Of course there is liable to be a dispute with those who think that one is not justified in calling the number yielded by this calculation an ' area '; though the defenders of the Meister-Möbius definition called it ' *the* right definition ' which ' alone is scientifically justified ' (R. Haussner's notes [1906], pp. 114-115). Essentialism has been a permanent feature of definitional quarrels.

Teacher: *Counterexample 4.*[1]

Beta: So that's the end of our conjecture. It really is a pity, since it held good for so many cases. But it seems that we have just wasted our time.

Alpha: Delta, I am flabbergasted. You say nothing? Can't you define this new counterexample out of existence? I thought there was no hypothesis in the world which you could not save from falsification with a suitable linguistic trick. Are you giving up now? Do you agree at last that there exist non-Eulerian polyhedra? Incredible!

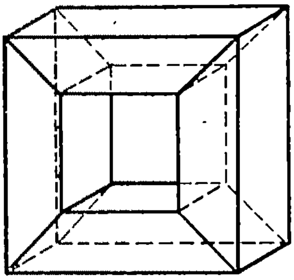

Fig. 9

Delta: You should really find a more appropriate name for your non-Eulerian pests and not mislead us all by calling them 'polyhedra'. But I am gradually losing interest in your monsters. I turn in disgust from your lamentable 'polyhedra', for which Euler's beautiful theorem doesn't hold.[2] I look for order and harmony in mathematics, but you only propagate anarchy and chaos.[3] Our attitudes are irreconcilable.

---

[1] We find *Counterexample 4* too in Lhuilier's classical [1812–13], on p. 185—Gergonne ain added that he knew it. But Grunert did not know it fourteen years later 327]) and Poinsot forty-five years later ([1858], p. 67).

[2] This is paraphrased from a letter of Hermite's written to Stieltjes: 'I turn aside a shudder of horror from this lamentable plague of functions which have no atives' ([1893]).

Researches dealing with . . . functions violating laws which one hoped were sal, were regarded almost as the propagation of anarchy and chaos where past ions had sought order and harmony' (Saks [1933], Preface). Saks refers the fierce battles of monsterbarrers (like Hermite!) and of refutationists that rised in the last decades of the nineteenth century (and indeed in the ng of the twentieth) the development of modern real function theory, 'the branch of mathematics which deals with counterexamples' (Munroe [1953], Preface). The similarly fierce battle that raged later between the opponents and protagonists of modern mathematical logic and set-theory was a direct continuation of this. See also footnote 2 on p. 24 and 1 on p. 25.

Alpha: You are a real old-fashioned Tory! You blame the wickedness of anarchists for the spoiling of your 'order' and 'harmony', and you 'solve' the difficulties by verbal recommendations.

Teacher: Let us hear the latest rescue-definition.

Alpha: You mean the latest linguistic trick, the latest contraction of the concept of 'polyhedron'! Delta dissolves real problems, instead of solving them.

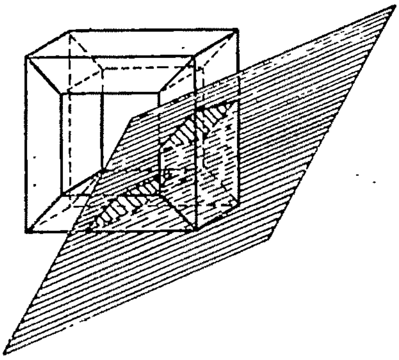

Fig. 10

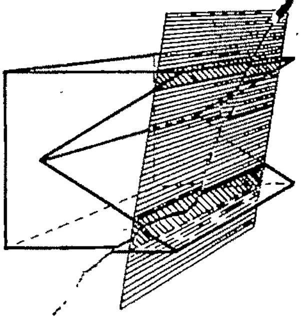

Fig. 11*a*

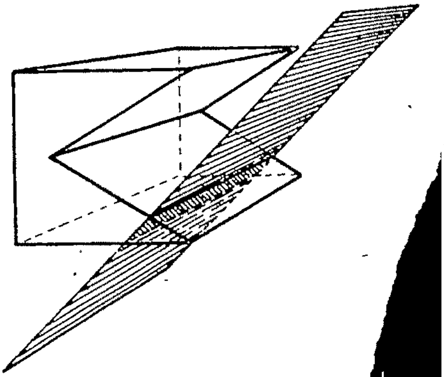

Fig. 11*b*

Delta: I do not *contract* concepts. It is you who *expand* For instance, this picture-frame is not a genuine polyhedron at

Alpha: Why?

Delta: Take an arbitrary point in the 'tunnel'—the space bounded by the frame. Lay a plane through this point. You will fin any such plane has always *two* different cross-sections wi picture-frame, making two distinct, completely disconnected polygons! (Fig. 10).

ALPHA: So what?

DELTA: *In the case of a genuine polyhedron, through any arbitrary point in space there will be at least one plane whose cross-section with the polyhedron will consist of one single polygon.* In the case of convex polyhedra *all* planes will comply with this requirement, wherever we take the point. In the case of *ordinary* concave polyhedra some planes will have more intersections, but there will always be some that have only one. (Figs. 11*a* and 11*b*.) In the case of this picture-frame, if we take the point in the tunnel, all the planes will have two cross-sections. How then can you call this a polyhedron?

TEACHER: This looks like another definition, this time an *implicit* one. Call it *Def. 5.*[1]

ALPHA: A series of counterexamples, a matching series of definitions, definitions that are alleged to contain nothing new, but to be merely new revelations of the richness of that one old concept, which seems to have as many 'hidden' clauses as there are counterexamples. *For all polyhedra* $V-E+F=2$ seems unshakable, an old and 'eternal' truth. It is strange to think that once upon a time it was a wonderful guess, full of challenge and excitement. Now, because of your weird shifts of meaning, it has turned into a poor convention, a despicable piece of dogma. (*He leaves the classroom.*)

DELTA: I cannot understand how an able man like Alpha can waste his talent on mere heckling. He seems engrossed in the production of monstrosities. But monstrosities never foster growth, either in the world of nature or in the world of thought. Evolution always follows a harmonious and orderly pattern.

GAMMA: Geneticists can easily refute that. Have you not heard [illegible]t mutations producing monstrosities play a considerable role in [illegible]croevolution? They call such monstrous mutants 'hopeful monsters'. It seems to me that Alpha's counterexamples, though monsters, are 'hopeful monsters'.[1]

---

[1] *Definition 5* was put forward by the indefatigable monsterbarrer E. de Jonquières [illegible]t Lhuilier's polyhedron with a tunnel (picture-frame) out of the way: 'Neither [illegible] polyhedral complex a true polyhedron in the ordinary sense of the word, for if [illegible]kes any plane through an arbitrary point inside one of the tunnels which pass [illegible]hrough the solid, the resulting cross-section will be composed of two distinct [illegible]ns completely unconnected with each other; this can occur in an ordinary [illegible]dron for *certain* positions of the intersecting plane, namely in the case of some concave polyhedra, but not for all of them' ([1890*b*], pp. 170-171). One wonders whether de Jonquières has noticed that his *Def. 5* excludes also some concave spheroid polyhedra.

DELTA: Anyway, Alpha has given up the struggle. No more monsters now.

GAMMA: I have a new one. It complies with all the restrictions in Defs. 1, 2, 3, 4, and 5, but $V-E+F=1$. This *Counterexample 5* is a simple cylinder. It has 3 faces (the top, the bottom and the jacket), 2 edges (two circles) and no vertices. It is a polyhedron according to your definition: (1) exactly two polygons at every edge and (2) it is possible to get from the inside of any polygon to the inside of any other polygon by a route which never crosses any edge at a vertex. And you have to accept the faces as genuine polygons, as they comply with your requirements: (1) exactly two edges meet at every vertex and (2) the edges have no points in common except the vertices.

DELTA: Alpha stretched concepts, but you tear them! Your 'edges' are not edges! *An edge has two vertices!*

TEACHER: *Def. 6*?

GAMMA: But why deny the status of 'edge' to edges with one or possibly zero vertices? You used to contract concepts, but now you mutilate them so that scarcely anything remains!

DELTA: But don't you see the futility of these so-called refutations? 'Hitherto, when a new polyhedron was invented, it was for some practical end; today [illegible]y are invented expressly to put at fault the reasonings of our fathers, and one never will get from them anything more than that. Our subject is turned into a teratological museum where decent ordinary polyhedra may be happy if they can retain a very small corner.'

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[1] 'We must not forget that what appears to-day as a monster will be to-morro the origin of a line of special adaptations. . . . I further emphasized the importa of rare but extremely consequential mutations affecting rates of decisive embry processes which might give rise to what one might term hopeful monsters, mon which would start a new evolutionary line if fitting into some empty environm niche' (Goldschmidt [1933], pp. 544 and 547). My attention was drawn to this by Karl Popper.

[2] Paraphrased from Poincaré ([1908], pp. 131-132). The original full text 'Logic sometimes makes monsters. Since half a century we have seen arise a of bizarre functions which seem to try to resemble as little as possible the functions which serve some purpose. No longer continuity, or perhaps co but no derivatives, etc. Nay more, from the logical point of view, it is these strange functions which are the most general, those one meets without seeking no longer appear except as particular cases. There remains for them only a very small corner.

AMMA: I think that if we want to learn about anything really we have to study it not in its 'normal', regular, usual form, but critical state, in fever, in passion. If you want to know the normal thy body, study it when it is abnormal, when it is ill. If you it to know functions, study their singularities. If you want to ow ordinary polyhedra, study their lunatic fringe. This is how ne can carry mathematical analysis into the very heart of the subject.[1]

But even if you were basically right, don't you see the futility of your ad hoc* method? If you want to draw a borderline between counterexamples and monsters, you cannot do it in fits and starts.

TEACHER: I think we should refuse to accept Delta's strategy for dealing with global counterexamples, although we should congratulate him on his skilful execution of it. We could aptly label his method *the method of monsterbarring*. Using this method one can eliminate any counterexample to the original conjecture by a sometimes deft but always *ad hoc* redefinition of the polyhedron, of its defining terms, or of the defining terms of its defining terms. We should somehow treat counterexamples with more respect, and not stubbornly exorcise them by dubbing them monsters. Delta's main mistake is perhaps his dogmatist bias in the interpretation of mathematical proof: he thinks that a proof necessarily proves what it has set out to prove. My interpretation of proof will allow for a *false* conjecture to be 'proved', i.e. to be decomposed into subconjectures. If the conjecture is false, I certainly expect at least one of the subconjectures to be false. But the decomposition might still be interesting! I am not perturbed at finding a counterexample to a 'proved' conjecture; I am even willing to set out to 'prove' a false conjecture!

THETA: I don't follow you.

KAPPA: He just follows the New Testament: 'Prove all things; hold fast that which is good' (1 Thessalonians 5: 21).

(to be continued)

'Heretofore when a new function was invented, it was for some practical end; to-day they are invented expressly to put at fault the reasonings of our fathers, and one never will get from them anything more than that.

'If logic were the sole guide of the teacher, it would be necessary to begin with the most general functions, that is to say with the most bizarre. It is the beginner that would have to be set grappling with this teratological museum . . .' (G. B. Halsted's authorised translation, pp. 435-436). Poincaré discusses the problem with respect to the situation in the theory of real functions—but that does not make any difference.

[1] Paraphrased from Denjoy ([1919], p. 21).

BETWEEN MICRO AND MACRO*

JOSEPH AGASSI

THERE are two acknowledged methodological demands which can be made of any newly proposed theory: it should yield the theory it comes to replace as a consequence or as a first approximation and also as a special case. The first demand amounts to nothing more than the demand that the new theory explain the success which the preceding theory had. The second demand amounts to the requirement that the new theory be more general and independently testable. Newton's yields Kepler's as a first approximation: assuming the interactions between planets to be sufficiently smaller than their individual interactions with the sun, one may conclude that planetary orbits are roughly ellipses. Also, the field of application of Kepler's theory is limited by Newton's in this way; for Newton's allows any conic section, not only the ellipse, to be the (approximate) orbits of solar satellites. The same holds concerning the relations between Einstein's theory and Newton's. The Einsteinian energy formula yields the Newtonian energy formula as a first approximation in the case of particle mechanics; it tells us that in this case, if only small velocities and gravitational fields are involved, the difference between the Einsteinian and Newtonian formulae may be too small to be detectable. This explains the success of Newton's as a particle-mechanical theory. Einstein's also limits the field of applicability of Newton's. For, asserting that field-energies have mass, Einstein's allows for the conversion of mass into kinetic energy of particles (which absorb light); whereas in Newton's mass is unalterable.

The question which interests me is whether similar relations hold between quantum mechanics and its predecessors. I shall present the standard answer to this question and discuss it in detail. The standard answer is definitely not the only possible one, but it is the only one I know of.

It is claimed that the classical theory—which means either some combination of Newton's and Maxwell's theories, or, preferably, the special theory of relativity—is applicable quite satisfactorily to systems so large that relative to their magnitudes Planck's constant may be

* Received 24. ii. 62

neglected. This claim is at least as old as the new quantum theory; it was promulgated by Heisenberg *apropos* his indeterminacy rules, and repeated by Bohr *a propos* of his principle of complementarity. Heisenberg's idea was that the indeterminacy of either the position or the momentum of a large body is usually much smaller than the limits of the inaccuracy of the measurements of these characteristics. Bohr's idea was that large bodies clearly manifest corpuscular characteristics and strong fields clearly manifest wave characteristics, yet when we descend to the micro-level both characteristics are manifest though, he contended, in a complementary manner. The Bohr-Heisenberg contention that quantum theory is applicable to the small and the classical theory to the large has been repeated by many writers since, and is now standard. I shall mention, in particular, two authors. In his interesting *What Is Life?*, Schrödinger asks the question, 'why are we so big?', and answers it, so as to live in the classical, quasi-deterministic domain, rather than in the semi-chaotic domain of quantum theory. The Bohr-Heisenberg contention has also been repeated recently by David Bohm (this *Journal*, 1962, 12, 265-80) who is particularly concerned with such questions both in his philosophical and in his scientific studies, whose significance I need not relate.

So much for the standard contention; and now to some objections to it.

There are some *prima facie* arguments against the contention in question. First, Rutherford's and Compton's ideas are purely classical and yet they provide important explanations on the micro-level. This, one may answer, may be construed not as a serious criticism, but rather as pointing out an oddity, an accidental applicability of a theory to a domain where it usually breaks down. The answer is not *a priori* objectionable, even though it is not particularly desirable. For the urpose of the present discussion is to see how much quantum theory plains the success of older theories, and here we are ascribing the cess to a kind of accident. Therefore, the *prima facie* criticism need [illegible] be taken seriously for the time being, but if the view that quantum ory is the domain of the small be rejected for other reasons, this cism may become more important in the sense that we may wish [illegible] new contention to take better account of Rutherford's and pton's theories.

Another *prima facie* criticism of the standard contention is that quantum effects can be amplified at will, so that quantum effects can intrude into the macro-level. This idea was initially levelled against the naive contention that the quantum domain, but not the classical domain, is indeterministic. And, indeed, against this contention the criticism is perfectly valid. Necessarily, if quantum effects are indeterministic and can be amplified, then even the macro-world is indeterministic. Yet one may claim that the one process—which produces a quantum effect—belongs to the domain of quantum theory since the effect is small, and the other process—which amplifies the one (quantum) effect into a macro-effect—belongs to the domain of the classical theories since its effect is large. Thus the criticism, *qua* criticism of the contention under discussion, namely that quantum theory is the domain of the small effect, is not valid; at least not obviously or clinchingly so.

For my part I see little need to enter into further detail on this point, since a simpler argument to the same effect is readily available. There exists a multitude of macro-effects which are explicable by quantum theory but not by the classical theories. For instance, consider the fact that gas flames turn from blue to yellowish when salt is sprinkled over them. This is as much a macro-phenomenon as any, and, we know, the classical theories do not allow for it, whereas quantum theory successfully explains it.

I shall discuss one preliminary answer to this argument, which is weak, before coming to a better one. To explain the yellowish effect, the preliminary answer would run, we have to refer to sodium atoms and emitted photons, which belong to the micro-world. This answer must be rejected: small as the emitted photons are, the production of them in high concentration allows us to describe some of their characteristics, say their wave characteristics, in classical terms, this in perfect agreement with the contention under scrutiny; yet no matter how high their concentration is, the classical theory cannot explain other of their characteristics, like their frequency, this not being in agreement with the contention under scrutiny.

The stronger answer to the criticism that some macro-effects [illegible] explicable only by quantum theory is as follows. We may remem[illegible] that the problem in hand is whether quantum theory explains [illegible] success of the classical theory, not its failures; and as long as quan[illegible] theory explains the partial success of the classical theory—its abili[illegible] account for the wave character of the sodium light—by the fact [illegible] the sodium emits a high concentration of photons, the explanation is satisfactory, and the failure of the classical theory to account for the colour of the sodium light is irrelevant here.

Accepting this stronger answer as correct, I wish to draw attention to the fact that it demands a reformulation of the contention in question: we should say, not that quantum theory applies to the micro-level and classical theory to the macro-level, but that wherever classical theory applies it applies because of the large-scale of the phenomena involved. This reformulation is acceptable in any case to most physicists, for they would declare that if we could solve the many-body problem within quantum theory we would be able to apply quantum theory to the macro-level successfully (and obtain classical solutions as approximations).

Yet the reformulation happens to strengthen a little the criticism which I previously set aside, namely that Rutherford's and Compton's theories are classical yet successfully applicable to the micro-level!

So far I have discussed only the methodological reasons for the wide acceptance of the standard contention, and I have tried to raise some difficulties with respect to those reasons. Bohr's theory of complementarity presents a completely different, non-methodological, reason. To present the problem clearly, I wish to start with Einstein's theory of 1905. According to that theory radiation consists of particles which behave in accord with the equations of Planck and Einstein, and only when aggregated in large concentration, behave roughly in accord with the classical wave-equations. It is well known that Einstein's theory is not satisfactory if for no other reason than the two-slit experiment: in this experiment even very low concentrations of the photons manifest wave properties. It is a very interesting historical fact that the two-slit experiment was first advanced as a thought-experiment, but I shall not go into that now. When de Broglie extended the wave-particle duality from photons to electrons, he ssumed that electrons are waves and explained the success of the eory that they are particles by equating the group-velocity of his ve-electron with the velocity previously ascribed to the (allegedly) ticle-electron. He thus explained the success of the corpuscular ry by showing it to be an approximation to a more detailed wave y. Yet de Broglie's theory does not work: like all waves his e should be capable of dissipation, whereas electrons keep their corpuscular integrity under all conditions (unless they are destroyed, which is quite a different matter). Is, then, the electron a wave or a particle? Bohr's complementarity denies a straightforward and simple answer to this question. In agreement with Bohr, most physicists today deny that we may ask through which slit in the two-slit experiment a given photon or electron travels. The question is awkward to answer because, to enable the wave-character of the electron to affect the result of the experiment, both slits have to be open, whereas if we insist that the electron is a particle with some wave-like properties it is difficult to envisage how the slit through which the electron does not pass affects the result of the experiment.

In order to exclude the question concerning which one of the two slits a given electron has passed through, physicists, e.g. Dirac, have asserted that the question is unscientific since we cannot test the answer to it experimentally: once we try to perform such a test we have destroyed the set-up of the two-slit experiment (by closing, in effect, one slit at least with some apparatus); and unscientific questions should not be asked: they are meaningless. This view is now the official doctrine.

It must be realised that according to the official view of quantum theory that theory asserts neither that electrons are particles nor that they are waves. If one wants to know what quantum theory asserts about electrons, one must use the language of quantum theory; it is when one tries to translate from this language to the classical language, that one gets into such difficulties as the one encountered when describing the two-slit experiment. Bohr's theory is that the classical language—and our intuition, which goes along with it—is a satisfactory tool for describing the macro-world, but not the micro-world. The reason we feel uneasy about, and ask illegitimate questions concerning, the two-slit experiment, is rooted in the limitation of the classical language and in the limitation of our imagination within that language.

The question of what facts can be satisfactorily described within the classical framework is quite different from the question of th success of the classical theory as an explanatory theory. For instanc the classical language suffices to describe the yellowness of sodium lig but it cannot allow for the explanation of that phenomenon: explanation involves descriptions of sodium atoms and photons.

It is not my purpose here to discuss the principle of complem arity, except in its relation to the standard contention. The prin of complementarity relates to differences between the domain describable by the classical and quantum theory, and the standard contention to the domains explicable by these theories. Yet there is obviously some connection between the two, which is not easy to point out; nor will it be very urgent to do so if a macro-phenomenon may exist equilibrium ordering, once identified, are wholly objective; in many realms the scope for discovery may be finite. If all this is true, here is food for thought.

Of course scientists must always be ready for surprises, perhaps even for the shock of realising that, as science advances, regions may grow in which there are no surprises left. For the history of the physical sciences shows that perfect intellectual adaptation to aspects of the environment is possible, so that no Einstein can ever have cause to distrust its Euclidean lucidity and certainty.

For many practical physicists, chemists, biochemists, and crystallographers this is perhaps obvious. The chemist, for example, has no doubt that the topology and symmetry of his visual images of the equilibrium forms of many simple molecules are objectively valid and final, in a sense in which the changing expressions of the advancing front of twentieth-century 'fundamental' physical theory cannot be, for its continuing progress shows that these expressions contain redundant, non-objective, and unduly restrictive assumptions. The benzene ring (1861–5) and the asymmetrical carbon atom (1874) form parts of a growing nucleus of definitive chemical knowledge. A gradual accumulation of empirical identifications has resulted in 'the logical step-wise construction of (organic chemistry) without the periodic upheavals which have occurred in the other sciences'.[1] It is curious that such definitely objective forms as simple molecules have during forty years been ascribed in turn to Lewis static octets, to Bohr orbits, to Schrödinger fields, to Hilbert operators—and to what will it be tomorrow? The chemist could make use of a geometrical theory of chemical symmetry expressing the facts which remain valid through ll the revolutions of fundamental physics.

But the successes of physical theory since 1900 in covering proses have depended on the use of mathematical expressions not ectly related to geometrical forms in visual space. It is true that, Bohr reminded us, it all comes down to the calculation of measurets made in a three-dimensional (3D) laboratory, and the visual s of high energy particles are strangely like classical 3D trajectories. he operator, matrix, and wave equations are too successful to be y mathematical devices for calculating something entirely dif; they must hold essential clues to fundamental structure. Thus estige of the fundamental expressions is continually drawing the

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[1] in *Centenary Volume*, New York, 1958, p. 83. I am indebted odd for a discussion of this point.

Mackay

attention of physicists away from what is visual and objective towards mathematical expressions of increasing abstractness and complexity. In consequence they have sometimes paid less than due attention to the certainties that have been established once and for all. If certainty is possible in some realms, this fact must hold lessons for theory.

Here is an example:

Crystals are of 230 distinct types, classifiable in a particular manner.[1] *The 3D arrangement of the atomic centres in every chemically and structurally homogeneous single crystal (where surface effects and dislocations can be neglected, and no external or internal forces are present) corresponds to one of the 230 geometrically defined infinite 3D space groups.*

This is, I suggest, one of the best established non-trivial numerical theorems concerning the universe. It is apodictic. It rests on lucid geometrical assumptions regarding static equivalences in infinite linear lattices of points; it has survived examination over eighty years; recent studies of dislocations, growth, and *non*-3D-geometrical groups [2] have strengthened it; the dislocations necessary for growth are discontinuities with respect to one of these 230 types; no one has found any reason for suspecting that the 230 might be wrong, indeed it is necessarily correct so long as men continue to see spatial objects as 3D. Thus the theory of the 230 groups provides a firm basis for the many developments of crystal theory which are already known to be necessary.

A distinction must here be drawn between two kinds of physical knowledge. Many classes of facts are firmly established. But it has been suggested that no physical theory can ever render fully explicit and precise the conditions under which it is valid. Newton could not know when relativistic corrections are negligible. Most theorie require a later meta-theory to bring out hidden, unduly restrictiv assumptions which render them merely approximate.

Yet this need not be so, if a severe condition is met:

An empirically successful theory must be built on the geometry of t dimensional Euclidean space (3DE). The basis and development of

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[1] Equally valid examples are the fourteen Bravais translation lattices, the t two classes of crystal symmetry, and the sixty-five regular point systems which their completion in the derivation (1885–94) of the 230 space-groups. S Jagodzinski, *Handbuch der Physik*, vol. 7, part I; *Krystallphysik*, 1955, p. 3 *International Tables for X-Ray Crystallography*, vol. i, 1952.

[2] H. Heesch, *Z. Kristallogr.*, 1930, **73**, 325, 346; A. L. Mackay, *Acta* **10**, 543; W. Holser, *Acta. Cryst.*, 1961, **14**, 1256. Dr A. L. me on the new 'black-white' non-3D groups.

theory must be fully explicit, its expressions must be such that they do not require to be transformed into approximate limiting cases of other expressions in order to cover the observations. This means (i) that it must employ concepts whose justification is provided by their consistency and correspondence to visually observed relations, and has not merely to be hoped for from some future axiomatic treatment of more general foundations; and (ii) that its mathematics must be ' pure ', i.e. employ only methods (enumeration, 3D Euclidean geometry, symmetry properties) tested and justified not merely by their internal consistency, but by their direct representation of visual observations, and avoid more complex constructions which may contain non-objective features.

These conditions are not as arbitrary as they may appear at first sight. The mathematics of discrete points and lines in 3DE contains no concealed assumptions of a kind which can prejudice its objective representation of *visual observations.* Thus the above condition is necessary for the branch of physical theory which is to possess apodictic certainty in relation to visual observations. This branch must avoid both complex constructions that may surreptitiously introduce doubtful assumptions (e.g. externally imposed (3+1)D co-ordinate frames) and non-geometrical concepts (e.g. electric charge). The 230 Rule is apodictic because it meets these conditions; quantum field theory is provisional only, perhaps partly because it does not. But it remains to be seen whether a general theory of particles and fields can be built on the geometry of 3DE.

The special character of the 230 Rule can be identified more precisely. Here a theorem in the ' pure ' mathematics of point arrangements in 3DE has been found to correspond perfectly to a class of static physical systems. It is this perfect correspondence of ' pure ' mathematics with visual observations which gives the Rule its high certainty. There is a fit between mathematics and fact which is unmistakable. Once this fit is certain, no doubt can arise about the Rule, for the mathematical theorem is reliable. The 230 are objective types to which all actual crystals tend to conform; they are not quantitative laws with arbitrary constants which can ultimately prove to be merely approximate limit cases of some more general law.

On a long view, remembering the unique historical importance of 3D geometry for physics, it would be surprising if the 230 Rule were not followed by others of equal geometrical certainty, such as a comprehensive combined periodic table of nuclei and of atoms based on geometrical theorems, and not involving electrical concepts.

Other examples, perhaps not far off, might be a general theory of the geometry of chemical and biochemical structures, resting mainly on symmetry properties; a geometrical theory of the factors in the genetic code for protein; and some development of Bernal's statistical geometrical theory of liquids.[1] These would constitute parts of a definitive theory of simple and complex 3DE structures, based on the accumulation of wholly objective discoveries. In this theory enumeration, the properties of particular integers in relation to 3DE, and symmetry, must together play an increasing rôle, and the theoretically arbitrary quantitative properties of constituent parts a decreasing one.

This brief analysis discloses a sharp contrast between the definitive character of the 230 Rule (and similar geometrical identifications) and the provisional nature of even the most reliable of the principles involving dimensional measurements. Consider one of the most penetrating principles of this century:

Light is always propagated in empty space with a definite velocity c *which is independent of the state of motion of the emitting body.*

But what is 'empty space' in a fundamental theory? (i) Vacuum polarisation effects, expressing a non-linearity of the electromagnetic field in matter-free space, suggest that a variation of c in high external magnetic fields may shortly be observable.[2] (ii) Such anisotropic non-linearity renders relativistic expressions ambiguous. (iii) c may not be necessary within nuclear distances. (iv) In a unified theory 'light' might only be separable from 'electrical particles' under special conditions. Thus the macroscopic invariant, c, as Einstein understood it in 1905, is unlikely to possess universal validity.

This is one example of many. Other invariance principles are also associated with particular dimensional constants whose theoretically arbitrary value awaits interpretation. It has been suggested[3] that in this heterogeneous, anisotropic, and changing universe such invariance principles may be better regarded as methods for selecting and classifying stationary parameters, than as absolute properties of actual processes. The next task is to discover, within a broader context, *when* invariance principles apply and *why* their associated constants have certain values. This requires a deeper insight not merely into spatio-temporal measurements but also into the geometrical basis of inertial *and electrical* properties, a problem which Einstein recognised but did not solve. It is evident that the present foundations of electrical physics are far from possessing the geometrical finality of the 230 Rule.

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[1] J. D. Bernal, *Nature*, 1960, **185**, 68–70

[2] T. Erber, 'Velocity of Light in a Magnetic Field', *Nature*, 1961, **190**, 25

[3] In discussions, and, for example, D. H. Wilkinson, 'Towards New Concepts'. Chapter VI of *Turning Points in Physics*, ed. A. C. Crombie, Amsterdam, 1959, pp. 163–164.

This contrast between definitive certainty in one realm and provisionality or approximate character in another is the product of a divergent tendency in physical theory since around 1860, two branches advancing in different directions: (i) towards increasingly abstract, provisionally 'fundamental' theories of process; and (ii) towards a definitive theory of static or stationary 3D structures by the accumulation of wholly objective identifications. The first branch proceeds by covering new and more accurate measurements under more complex symbolisms; the second relies mainly on tracing 3D equivalences and symmetries.

One consequence of this 100-year divergence, mainly since Maxwell's equations (which are challengingly non-geometrical, since they employ a pseudo-scalar of one arbitrary sign), is that the relation between fundamental theory and observable 3D structures has become increasingly complex. This presents theoretical physics with the possibility of *two alternative or complementary paths* of advance, an unprecedented situation. It can *either* remain methodologically orthodox, i.e. develop and test more complex mathematical abstractions; *or* it can attempt a revolution in method and seek to re-interpret and to provide a fresh representation of the global relations of the experimental systems in which the high energy particles are observed,[1] not merely of the individual particles taken in isolation; *or* it can use both ethods. Both aim at more powerful methods of approximation. t while the first leads to the separation of smaller and smaller particles structures, the second would endeavour to treat the observed icles as functions of the total quasi-macroscopic experimental tion reinterpreted in terms of a new method absorbing the lessons ativity and quantum theory.

hus we find ourselves led to repeat our earlier question, this time pecial context: *is anything already definitively known regarding these mental systems, so that it constitutes an indispensable part of any*

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rious efforts are being made to provide a new representation of physical and processes and a method of quantisation appropriate to very small distances and high energies (D. Bohm, E. J. Sternglass, and others) but it is too early to estimate their value.

theory? Yes, their discrete structure. *It is certain that at many levels (including those accessible to visual observation) physical systems possess an 'atomic' structure.* Physical observations of atoms and molecules certainly require atomic interpretation. Whatever the ultimate basis of a unified particle theory may be, continuous or discrete, finite numbers of atoms (or baryons) are present in every finite system. This is definitive physical knowledge, and its mathematical expression is apodictic: *At the levels of observation at least one characteristic length* (l_0) *(or the equivalent) is indispensable to determine the finite spatial relations of the finite number of atoms present in any observed system, for example to fix the scale of stable equilibrium forms.*[1] It is the presence of such an l_0 that distinguishes true physical atomism from Cartesian pseudo-atomism (with infinitely small particles) and from any true continuum theory. Thus any future unified physical theory must certainly employ an l_0, or its equivalent. This conclusion is, I suggest, as certain as the 230 Rule; it is indeed implicit in the correspondence of the 230 groups to actual crystals of finite size.

If, as suggested, there exists a steadily increasing body of definitive physical-geometrical identifications, benefit may come from treating them as a special branch of physical theory which we may call *3D-geometrical discrete morphology*. The main assumptions of this branch are the 3DE character of visual space and the atomic constitution of physical systems. So far it has been concerned mainly with the properties of point groups, of space groups, and of arrangements of particular finite numbers of points, and it has not yet treated *changes* of any kind. While inertial and electrical principles must apparently be regarded as steps towards the more comprehensive conceptions of a unified theory, the identifications of 3D-geometrical atomic theory, once full confirmed, become an inalienable part of exact knowledge.

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[1] The necessity in any atomic theory of rigid bodies for at least one const length was recognised by R. J. Boscovich (1758, see *R. J. Boscovich*, edited L. London, 1961, p. 116); by J. Larmor (1900, *Aether and Matter*, pp. 183-19 L. L. W. on Larmor, *Nature*, 1960, **186**, 1010-14); and emphasised by L. L. (*Zeit. f. Physik.*, 1929, **56**, 809; and 1954, 'On the History of Natural Le *Annals of Science*, **10**, 10-27).

DISCUSSION

Discussion of Professor F. A. Paneth's Second Article

It is quite true I am sure that, as Professor Paneth puts it, ' the important advance in Lavoisier's definition of an element lay in inferring the existence of a basic substance from each simple substance found experimentally '. It is also true that there is no major change in the metaphysics of chemistry from Aristotle and Paracelsus, through Boyle and Lavoisier, and right on to Berzelius and Cannizzaro. With regret, I must also agree with Professor Paneth that M. M. Pattison Muir made an important mistake in his general way of interpreting *The Sceptical Chemist.* (I say with regret, because I have learned so much from the writings of Pattison Muir.) It is rather curious that neither Professor Paneth in his two articles, nor Professor Marie Boas, in her book on Boyle, quote the following passage, which I think was mainly responsible for Pattison Muir's mistake: ' . . . ' tis now time to consider, not of how many elements it is possible that nature may compound mixed bodies, but (at least as far as the ordinary experiments of chymists will informe us) of how many she doth make them up ' (from *The Sceptical Chemist.* The Third Part). Pattison Muir I think would have paraphrased this: The number of chemical elements cannot be determined *a priori.* It is a question for the future, and will be determined *a posteriori* by experiments. From this it would have appeared to him but a small step to equating the ' basic substances ' and the ' simple substances found experimentally'. But it is quite clear from a careful study of the whole of Boyle's book, that in fact this step was neither intended nor taken by Boyle. We are justified in stating that Boyle's great advance lay in the understanding that the necessary number of basic transcendental substances must also be found *a posteriori* from experience. (I dare not have used the word ' transcendental ', had not Professor Paneth himself done so!)

To Lavoisier himself, as Professor Paneth makes delightfully clear, the ' base of oxygen ' is quite hidden from experience and is utterly ' transcendent '. In chapter IV of the famous *Traité Élémentaire* (1789) Lavoisier has this to say about oxygen gas d its ' base ': ' The union of this base with caloric, which is the same with what s formerly named *pure,* or *vital,* or *highly respirable air,* we now call *oxygen gas.'* bert Kerr's translation.) The titles of Lavoisier's chapters V and VII are most ructive:

Of the Decomposition of Oxygen Gas by Sulphur, Phosphorus, and Carbon— . . . '

Of the Decomposition of Oxygen Gas by means of Metals, . . .'

n Lavoisier refers to oxygen being present in all the acids and bases, he certainly not mean that oxygen gas is present in them. He means precisely the same as sor Paneth means by ' basic substance '.

ere is something that could be added to all this. Lavoisier and Kant both n great debt to Newton. Suppose we write the ' chemical equation ' for the reaction which Lavoisier himself made so famous:

$$2Hg + O_2 = 2HgO.$$

It is a bad tradition of the chemistry teaching of this country to see in this kind of formulation, first and foremost, an expression of the atomic theory. This is a comparatively trivial matter, and we should not accept our chemical philosophy from school-boys at Ordinary Level. Much more central to the meaning of the chemical equation is the Kantian reference to that which alone can change, because it is permanent. And what in fact is this? It is Newtonian mass, the same in the red crystals as in the silvery metal and the oxygen gas. 'Mass', as I pointed out in my previous note, was in effect added to the Cartesian list of primary qualities by Newton and Locke. Mass, by definition, lies right outside the realm of any possible experience. For example suppose we *see* that a gold ball A looks bigger than a gold ball B. Can we judge

$$m_A > m_B?$$

No, ball A may be hollow. But, suppose A *feels* heavier. This, properly, simply gives me

$$m_A g > m_B g,$$

and by a *mediate* inference,

$$m_A > m_B.$$

Whatever I do with the gold balls, I can have no *immediate* sense-perception that

$$m_A > m_B.$$

This is the precise meaning which the philosophy of Kant and the physics of Newton impart to the Cartesian doctrine that 'weight' is a secondary quality, and to the doctrine of Locke that 'mass' is a primary quality.

Professor Paneth is absolutely correct when he says that 'the concept of basic substance as such does not in itself contain any idea of atomism'. It is something transcendent, and—I would add—so is its mass. My colleague, Dr J. J. Kipling, was discussing recently with me the answer of an 'honours chemistry' student to the question, 'What is an isotope?' The answer began, 'An isotope is an atom, which . . .'. For such students, Professor Paneth's two articles should be compulsory reading.

John Bradley

The Editor,
British Journal for the Philosophy of Science

Sir,

In my recent review of 'The Threefold Cord' by Lord Samuel and Profes
H. Dingle (13, No. 52, p. 339) I ascribed to Sir H. Tizard the view that 'A theory
the aether (Lord Samuel's) expressed in words seems too vague to be of much
and I don't see how you would put it into symbols'.

Professor Dingle has kindly pointed out to me that I misread what the late
Samuel wrote (p. 230). This comment was made by 'an eminent British phys
whom Sir Henry had introduced to Lord Samuel's book 'In Search of Re
The identity of the physicist was not disclosed but Professor Dingle believes th
Henry would not have shared his views.

I was also mistaken in ascribing a sentence of Samuel's to Dingle, who do
consider the question of action at a distance as 'logically settled'.

Yours faithfully,
G. W. Scott Blair

REVIEWS

EVOLUTIONARY MAN

I *The Moribund Supernatural*

IN the medieval world (and even more so, in the Dark Ages) it was presumed that anything could happen [1] at any time according to the arbitrary fiat of god or demon. For even physical phenomena, it was thought, whether they normally behaved according to ' laws ' or not were not *bound* by any science but were at the mercy of spirits, or could at least be dominated by the arbitrary will of God.

But during the past 300 years and more, with the rise and development of the physical sciences, men have increasingly come to the conclusion that events in the physical world occur according to inviolable and discoverable laws. Whatever be the power of God or Devil in whatever their sphere of influence, we have become increasingly sure that they do not in fact fortuitously ' break in ' on physical nature causing unpredictable things to happen. As Spinoza was at pains to show ' if a stone falls from a roof on the head of a passer-by and kills him ', this is due to a train of perfectly understandable natural causes. To say, as used to be said, that the tragedy was the will of God was idiotic. For ' the will of God ' was only ' the asylum of ignorance '.[2]

Nevertheless it was assumed, until quite recently, that this science-centred pattern of human thinking about the physical world was an order of things created and sustained by an external supernatural authority. The order of nature was under Divine auspices. God *could* fortuitously ' interfere ' *if* He wished. If and when He did we had *miracle*. It was also ssumed by most Western people until quite recently (as it is still assumed by hatever number of Orthodox Believers still remain) that in addition to d potentially *able* to tamper with the physical universe there is that same d who not only can, but does, continually interfere in a spiritual order of gs. Thus if a man is cured of some disease after his having prayed God ecovery (and especially in a case where the medical practitioners have

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Of course as Lecky points out phenomena were not thought of as being all *equally* to exhibit unaccustomed and unexpected behaviour. Some are more unpredict- han others and ' appear capricious ', and it is these that ' are associated especially ivine intervention '. Prominent examples he gives are comets, meteors, and general) atmospheric phenomena. *History of Rationalism in Europe*, London, 1946, p. 102.

[2] Spinoza's *Ethics*, Appendix to Part I

given him up as a 'hopeless case'), the believer in a *spiritual order*, as traditionally understood, certainly ascribes the cure completely to the will of God.

But more and more of late, educated thinking men in a climate of opinion increasingly scientific and encouraged by the ever growing industrialisation and urbanisation of their mental 'atmosphere' have become sceptical. Sceptical as to whether, in fact, there is any supernatural independent order of things over and above and beyond our human experience.

There are, perhaps, four main reasons for this increasing scepticism:

(1) As the sciences gradually increase in number as well as scope, it is realised that more and more domains of human experience, once conceived of as not-to-be-explained by any science, are yielding their secrets to science. This naturally leads men to the thought that possibly *all* '*mysterious*' phenomena are only mysterious because of human ignorance. So it seems possible, or even probable, that when we have come to explore and explain the *Supernatural* more fully, through the evolution of scientific method, it will be discovered to be *natural*.

(2) (closely linked to (1)). If there were a supernatural world existing outside and independently of the natural, it would indicate a dualistic universe. And this is repugnant to human reason which in the last resort inherently demands a monistic universe. It makes more sense.[1]

(3) Modern astronomy is making it increasingly evident that the traditional source of the Supernatural *in some place* 'beyond the stars' becomes less and less likely. And although one can plausibly make out a case for God and Heaven being 'located' *within* human personality, they appear less awesome than the external anthropomorphic Abode from whence man traditionally derived his being. In a word, astrophysics says to man today: 'If you are to use the terms God and Heaven with any correct (sensible) meaning, you must necessarily be referring to the highest (so far) *human* experience and *human* values.'

(4) The behaviour of the traditional Supernatural Deity is so erratic,
inconsistent, and unjust that it seems unlikely that He has any existenc
outside the imaginations of individuals. His behaviour? Take, for i
stance, His reaction to humanity's petitionary prayers. History seems
indicate clearly that even if petitionary prayers were ever 'answered' (
that the particular events were not just due to natural causation), they v
answered so capriciously, and so independently of the moral worth o
petitioners, that all unprejudiced rational men are bound to conclude
the supposed Deity is without conscience as well as without heart.

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[1] It seems obvious that Western philosophy would have tended, all through its
years history, to be always monistic (though doubtless under varied forms) had
been for the mental and moral inhibitions of the philosophers. For nearly all of the
were *prejudiced* by their religious education and so were pre-determined to think dualistically.

Or take human suffering. This He permits without apparently any consistent and just plan. Briefly, the 'good' may die in agony; the 'evil' may live in long enjoyment.

In earlier days men accepted these facts, as well as the God who supported them, because theologians and their satellites (all of whom were very wise men) assured the less wise that these things were true and even just. Fear paralysed the natural reasoning power of those unversed in the Queen[1] of the Sciences. But as men have become more mentally and morally emancipated through the spread of education this fear has increasingly diminished, and as it has diminished there has developed a growing sense in the individual of a freedom and a right to think for himself. In this process he becomes aware[2] that his moral consciousness is in revolt not only against the naïveté of traditional religious dogmas and doctrines but also against their not-sufficiently-edifying tone.[3]

It thus seems to him more healthy, as well as more reasonable, to venerate the more noble of the moral and spiritual elements that he finds within himself rather than continue to pay homage to the gratuitous, and often capricious, Demiurge of the theologians.

For all these reasons then civilised man has become more and more sceptical about the value as well as the truth of traditional religion. And it is against the foregoing background that one can perhaps best try to review and assess *The Humanist Frame*.[4]

2 *Humanist themes*

The book is a symposium of twenty-six essays by some twenty-seven contributors. The basic thought which holds the diversity of the chapters together is this: 'Man's destiny is to be the sole agent for the future evolution of this planet' (p. 17). And this he must achieve 'unaided by outside help. In the evolutionary pattern of thought there is no longer either need or room for the supernatural' (p. 18).

Each chapter deals with some particular aspect of this general theme. r example, man (as individual and as society) is examined in relation to h of the following: medicine, the sciences (in general, and in particular), tics, economics, education, international integration, morality, the fine religion, world resources and population, scientifically controlled ding, and even Outer Space.

.e. Theology

This is a rationalisation. It should be conceded, without any impairing of the ent above, that few even among the emancipated think thus *consciously* and *ex-* More often the revolt is instinctive and subconscious.

T. A. Sinclair in *A History of Classical Greek Literature*, London, 1934, p. 282, 'Human beings are often nobler than the gods they worship'.

[4] *The Humanist Frame*, edited by Julian Huxley, Allen and Unwin, London, 1961, pp. 432, 37s. 6d.

And since all the contributors to this symposium are fully conscious of the fact that they see man and all his activities (science, morality, education, etc.) as essentially *evolutionary*, they properly have all the time an eye on human history—past and present, but especially on the expected shape and complexion of the *future* history that is in process of being evolved. For the avowed aim of these Humanists is to try to give some indication of the way in which humanity may *improve* itself in the future through conscious and deliberate *planning*. At the same time the planning must be realistic, i.e. what man can become must be based on a full realisation of his *nature*, past as well as present.

For reasons that will be manifest later [1] in this review, no attempt will be made to give any *complete* summary of the contents of this book. We merely outline what seem to be the leading ideas that keep cropping up; and for this present review a 'leading idea' will mean not only one which would seem to have practical relevance to evolving man, but also one that is scientific, or is at least in large measure conducive to scientific treatment.

The Preface and the opening chapter are each from the pen of Sir Julian himself. The gist of the book (as he says in his Preface) is easy to summarise. Thus: Evolution has not been one continuous homogeneous process. It has come about in distinct phases—(1) inorganic, (2) biological, (3) psychosocial. Mankind is now entering (4) *self-consciously purposive evolution;* in this phase man plans his own destiny, so we have the phase of *Humanism.*

Then follows the first chapter which is itself entitled 'The Humanist Frame', and which is written with a high degree of lucidity and which serves as an excellent introduction to the varying themes of the contributors who follow—in fact many of their ideas are 'touched on' by Sir Julian in anticipation.

The Humanism that is here being sponsored is, he insists (p. 14) 'necessarily unitary instead of dualistic, affirming the unity of mind and body; universal instead of particularist, affirming the continuity of man with the rest of life . . . naturalistic instead of supernaturalist, affirming the unit of the spiritual and the material; and global instead of divisive, affirmi the unity of all mankind.' And again, his humanism will have nothing do with *absolutes*. Truth, morality . . . and authority are all rela This last seems obvious since (as he goes on to say), 'This new visi inevitably an evolutionary one.' And it becomes abundantly clear i course of the book that evolution is *total; everything* evolves. So of c there can be no absolutes.

But Sir Julian stresses the importance of the fact that man's evol from the time of his emergence 'as truly man' (p. 16) has no longe biological but has been, first, *psychosocial*. This psychosocial phase from the birth of conceptual thought and language which (both individually and socially) has brought about *continual improvement* through the elaboration and transmission of ideas. This phase is a great advance in man's development. But it has its limitations in that these ideas have often been gratuitous dogmas and prejudices, antithetic to change and progress.

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[1] See Section 3, p. 49

But now (stemming from Darwin) mankind is entering a new phase, that of *self-consciously purposive* evolution. The movement of mankind from the biological, through the stages of the psychosocial, and leading into the self-consciously purposive is summed up:

> 'There is thus a succession of successful idea-systems instead of a succession of successful bodily organisations. Each new successful idea-system spreads and dominates some important sector of the world, until it is superseded by a rival system, or itself gives birth to its successor by a breakthrough to a new organised system of thought and belief. We need only think of the magic pattern of tribal thought, the god-centred medieval pattern organised round the concept of divine authority and revelation, and the rise in the last three centuries of the science-centred pattern, organised round the concept of human progress, but progress somehow under the control of supernatural authority. In 1859 Darwin opened the door to a new pattern of ideological organisation—the evolution-centred organisation of thought and belief.' [1]

This 'evolution-centred organisation of thought and belief' has had *tremendous consequences*, which are clearly grasped by the contributors generally (following Sir Julian's lead). They can be expressed in this way: (1) Man has been 'ousted from his self-imagined centrality in the universe' (p. 18) to find himself in an infinitely insignificant location in a million galaxies. (2) On the other hand, through the concept of evolution, man is infinitely significant. [illegible] '*mind*' and 'the evolution of mind . . . is an extremely rare e[illegible] the . . . universe.' Sir Julian implies ([illegible] man can now be [illegible] in

It is this. In 1859 Darwin appeared (to those who bothered to think about him at all) to be contending by his theory of evolution that man was not specially created at a specific moment in time, but that he in fact gradually evolved over a vast period from more primitive forms of animal life. It appeared that, revolutionary as the theory seemed, the matter simply concerned biology and could be contained therein. Other sciences and departments of knowledge could apparently 'carry on' pretty well as before—proclaiming their Absolutes, their static facts, their unchanging data. Thus theologians, although not too happy (as the controversies showed) about the scientific attack on man's sacrosanct origins, and not always sure as to how they ought to react to Genesis I, at least believed almost to a man that the fundamentals of theology were quite safe and untouched by the Darwinian heresy. Even if evolution *were* true, God (the Great Fundamental) was still 'there'. Even evolution did not create itself. It needed God to do that. God (the Permanent) started it off as part of his plan.

But now, in fact, the limiting of the concept of evolution to biology has gone except in the case of 'those who deliberately shut . . . their eyes (p. 17)'. Or, let us say, the concept has expanded right out beyond the bounds of biology until now (as the Humanists clearly recognise) it is seen to apply right through the universe in every department and aspect. As Sir Julian has written elsewhere [1]: 'Since Darwin, every field of study, from astronomy to human history, from geology to linguistics, must be viewed *sub specie evolutiones* [evolutionis?], as a process.' That is to say *change* and *development* are the very heart of the being of the *whole* universe. The growth of man's knowledge in *every* department depends upon our ability to discard, at every stage, more naïve concepts for those that are more sophisticated. *Theology* will dou[illegible] some vital meaning for humanity. B[illegible]plied in Humanism) accepts the i[illegible]nge, and advances [illegible] them all the fundamentals were '*fixed*'. For the Humanists, because of the fundamental concept of *change*, man is indeed Prometheus. All this is vital to the book under review and permeates it.

Man's future programme. What are to be the present aims of this *changing man* in the light of his ' new evolutionary vision ' ? How to build a better world ?

(1) Man's thinking must be global. We must transcend nationalism and pool our resources. Otherwise we shall fail to achieve the highest that humanity is capable of; for energies will be dissipated in ideological conflict.

(2). And yet this must not be allowed to mean the triumph of Ad-mass. We must be concerned for the individual. ' Though our new idea-pattern must be unitary, it need not and should not be cramping, or impose a drab or boring cultural uniformity. . . . Cultural variety . . . is the spice of life ' (p. 22). And so it is essential to preserve it. Particularly in education, we must encourage individual variety and not try to educate children as a homogeneous mass. They are not equal in their gifts and potentialities. The motto must be ' free but unequal ' (p. 24).

(3) We must face the population-explosion and deal with it drastically. Too many people are being born and survive, and the rate of increase is out of all proportion to the world's resources. But we need not only universal birth-control, but also, by the application of science, to breed only the best types. In other words, world-population must be reduced in quantity and increased in quality.

(4) The Western eco[illegible]nd needs a ' reversal of ideas ' (p. 24) It i[illegible]tinually [illegible]

aesthetically effective form'. Art is not a *thing* but a 'type of human activity and its products' (p. 27). It is, in a sense, a way of 'escape'—but if we say this, it is not an escape 'downwards' (as if through drink or drugs) but upwards to 'new levels of being, where we make contact with something more enduring . . . and . . . higher than is to be found in the world of material needs and everyday routine' (p. 29).

(2) *Science.* He starts off with an apt warning. 'We must beware of the misuse of words like *science* and *scientific*, especially by those who want to cash in on the prestige of science to advance their own views or interests. Thus theology once arrogated to itself the title of Queen of the Sciences, and still claims rather plaintively [1] to be a science—a claim which it could only justify by adopting scientific method.'

Science is tending more and more to present us with a monistic universe. The separate sciences grow and frequently fuse with one another to form a more comprehensive pattern. Each science, or patch of organised knowledge, is as it were an island in a sea of ignorance. But as the *whole* of human experience becomes gradually organised there will be no more isolated islands for they will continue to join up until at last the sea of ignorance will have disappeared. Sir Julian, as a Humanist, takes the view that *all* phenomena are at least potentially capable of being investigated by the methods of science. Even 'values' can and must be scientifically studied.

(3) *Religion.* Theism must [illegible] implies a dualistic theory of the universe, i.e. 'the [illegible]ssential cleavage between natural and supernatu[illegible] Theism is the enemy of pro[illegible]? While

rightly concerned with the conviction that the Humanist world which is being more and more realised as an actuality depends in many respects on a World-View, on a greater integration of knowledge as well as of attitudes. What we need is to see more and more clearly all the separate departments of mind and of human life not any longer as separate isolated 'islands' of knowledge and of living activities but as organic parts of a unified whole; each part, because of its organic relationship with its 'other', implying its coexistence and thereby completing its own *meaning*.

What is written in 'The Democratic Challenge' does stand as true (or mainly true) on its own. And what is written in 'Humanist Economics' also stands as truly on its own. But a more useful, because more comprehensive and more fully integrated, insight into the total politico-economic world situation might well have been expected to have emerged had these two able contributors got together. It cannot be enough to leave it to the reader to make the necessary integration here.

(*c*) Last, but by no means least, the book is dominated by a most curious enigma. The name of a Jesuit Priest (the late Teilhard de Chardin) is frequently introduced into the pages so that one [1] of his chief works may be either quoted or paraphrased, or (at least) be commended to the reader as a work that he would do well to study. We have to bear in mind (1) that these humanists explicitly reject any allegiance to supernatural religion, and (2) that Teilhard de Chardin in *The Phenomenon of Man* is not concerned merely with some secular non-theistic theme but that, in fact, he is explicitly concerned to vindicate not merely a general theism but specifically the Christian Religion.

Now it is to be admitted that only a relatively small part of this book of the Jesuit is occupied with theistic argument; most of it is an exposition of cience, much of which is sufficiently valid. And the contributors to *The umanist Frame* who use Teilhard's name do so almost exclusively with erence to science rather than to theology. None the less, the situation is t a little curious.

Firstly, because in the chapters where Teilhard is either paraphrased or ted with reference to his science, there is nothing really original, noth-hat is not known to other scientists (i.e. biologists) independently of rd's work.

condly because the Priest seems in most of these instances, if not in all, ntroduced *artificially* (see pp. 73, 200, 242) and in such a way as to t that these humanists are not too sure of themselves and are using d not because his work is intrinsic to the humanist argument but they feel the *need* for an external independent Authority who can ded as a revered Sponsor. Can this be the case? If the Priest's d been used only once or twice one might reasonably console oneself with the thought that there is no accounting for tastes, that even pukka intellectuals can occasionally enjoy fleeing monotony in a capricious prank. But the name of Teilhard occurs at least eleven times. A sort of ritual? A talisman? Entertaining (perhaps) if one likes theatre. But incantation should not have found a place in an otherwise sober, dignified, and objective work.

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[1] *The Phenomenon of Man*, London, 1959

But finally (and even more important than either of the two foregoing considerations) one would surely have imagined that finding accommodation for a *Priest* in *The Humanist Frame* would be a psychological and moral blunder—the very idea of such a person, quite apart from the relevance or irrelevance of his subject-matter! For, after all, this book is avowedly concerned with being a vehicle of communication of man's 'new revelation'. It is to help (*at least* 'help'?) to show him 'a new vision of his destiny' (p. 6). It aims at clearing the mind of superstition by indicating (many times) that the traditional supernatural religions have been outgrown, at least by thinking people.

Now one is safe in assuming that apart from already professed humanists, most people who read this book will not be professing Christians. They will be thinking people who are keen to know the truth as to what man *is*, and what man *can do*, and who have already come to the conclusion that all theistic religions, and their official representatives, are far too naïve to be sufficiently true to satisfy the more 'adult' contemporary minds.

Well, what are they pretty certain to think as they try to assess *The Humanist Frame?* That they have discovered a work which, on the whole, is sober, sane, rational, but one which is also greatly marred by the seemingly inexplicable fact that these authors, the professed sponsors of the *new revelation*, eleven times invoke a 'Priest'—an official Representative of the *Old Revelation*.

Let us face facts, and not be 'ostriches'.[1] Many educated honest trut seekers, who know something of the last 2,000 years of human histo cannot avoid associating 'priest' with 'priestcraft' and 'priestcraft' w 'falsehood'. We have here a book, that could have been excellent, sp For all their professions about looking to the future, these humanists, Lot's wife hankering after Sodom, just *have* to *look back*.

And yet, notwithstanding the above criticisms, *The Humanist Fr* important. Its publication is, as it were, a crucial point in the evolut Human Society. For it is the first time in thousands of years of b Theism, and of all the superstitions associated therewith, that a b people, distinguished for their learning in their several fields have k temerity to get together and express a disbelief 'in supernatural c rulers, or influences.' And at the same time expressing their fait ability of mankind to achieve the progressive betterment of the hu through reason and science. Nay more, many of the contributors state in varying ways that not a few of humanity's past ills have been either due to, or sustained by, Religious Belief.

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[1] Sir Julian's exhortation on p. 21.

All this must be regarded as a landmark in man's development. And the important thing here is surely that it is *not so much a landmark in the history of human thought as a landmark in the emancipation of the human spirit.* The very appearance of *The Humanist Frame* is significant of a new freedom—not just of mind, but of the whole man. For here we have men and women who are not merely daring to think ' all out ', without the traditional inhibitions bred out of superstitions, but who also dare, in their happy courage, to *say* what they think.

This is new, new in the history of so-called Western or Graeco-Christian Civilisation. There were, to be sure, people who thought freely before—in much the same way (in general) as do these present-day Humanists. In the pre-Christian world there were Epicurus and Lucretius and their followers. And in the so-called Christian centuries (nearly 2,000 years) there were from time to time, to be found, even in ' Christian ' countries, *individual* thinkers and writers who proclaimed freethinking humanist doctrines. This is admirably illustrated by Mrs Margaret Knight in her *Humanist Anthology*. But *The Humanist Frame* is, in certain important respects, unique.

It is not only a *new* challenge to old-fashioned theistic ways of thinking about man and the universe in the sense that this present challenge is based on more and better science than was available to humanists of the past. Although this is true and significant. It is also, and just as vitally, a challenge to traditional thinking in that these twenty-six reputable contributors are not only in obvious agreement among themselves about fundamentals, but also *because they are thinking and writing, for a vast number of people* who, lthough they feel[1] that many doctrines and traditions are false (being based [illegible] ignorance and superstition), have not got the education or ability to rk out for themselves just how mankind and life and the universe must viewed in the light of contemporary science. It is obvious therefore [illegible] even if at some (distant?) time in future history, many of the chapters [illegible] *e Humanist Frame* will be viewed by our posterity as containing obsolete [illegible] e and mistaken opinion, that the work will not appear even then as [illegible] ss of a landmark[2] in the history of the fight for freedom of thought.

Alan L. Stuart

[illegible] is needs no proof. One has only to bear in mind such facts as that most people [illegible] attend Church any more (except for very special occasions); or again most [illegible] ave ceased to believe that national calamities are to be attributed to the will of [illegible] They assume a *natural* cause.

[2] There were rights and wrongs in ' the fall of the Bastille ' in 1789. But that event is today as ever not any less of a landmark on a road towards political and social freedom.

REVIEWS

MID-CENTURY DEVELOPMENTS WITHIN PSYCHO-ANALYTIC THEORY

AN unusual work has appeared in the International Psycho-Analytical Library.[1] It is a long work devoted almost entirely to discussion of psycho-analytical theories with a view to sifting out inconsistencies and pointing to the form of theory that the author considers most satisfactory. Such a work is unusual in that discussions like this in a book have never taken place before in this field, and papers in the specialised journals rarely take this form either. A further unusual characteristic is that while most criticisms of psycho-analytic theories have come from people who did not understand it very well, though there have been some exceptions, this work contains the first detailed criticism made by one who does understand the subject thoroughly. The work is of general interest in two ways. First, the issues discussed involve methodology. And second, for those who think of psycho-analysis solely in terms of Freud, either undiluted or heavily watered down by neo-Freudians like Horney, the present work rectifies the picture. It is closely concerned with developments that are in line with the basic psycho-analytical approach in the sense of looking for endopsychic theories, so that they are in the spirit of Freud though on a small proportion of issues they differ from him very greatly. The best known of these are due to Melanie Klein, but it is not at all widely known that Fairbairn has also made an interesting critical attack upon theoretical problems and puts forward a radical revision of one of Freud's central theories. The overall aim of the book is to put Fairbairn squarely on the map and portray Fairbairn's work as a major system; all the discussion may be taken to be subordinated to these ends.

In order to embark upon critical discussion the author wisely goes t
some trouble to begin by stating the various theories as well as he ca
Methodologically this is essential, partly because one cannot criticis
doctrine unless one knows what the doctrine is and partly because psyc
analytical theories have never been explicitly stated in full detail.
nature of the exegesis is not methodological but in the terms that have
customarily used, and the author—again wisely—makes extensive u
quotation. This may be unfashionable but there can be no doubt ab
appropriateness here. The book contains about a hundred pages of Fr
exegesis which, however, has a slant to which I shall return; then
seventy pages on various forms of sociological methods of watering
the theory; and then we embark on a detailed consideration of the r
of Melanie Klein's work to Freud, a full account of some of her th

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[1] Harry Guntrip, *Personality Structure and Human Interaction: the Developing* *of Psychodynamic Theory*, The Hogarth Press and the Institute of Psycho-Analysis, London, 1961, pp. 456, 45s.

the further developments and modifications of both Freud and Melanie Klein that are necessary in the author's view, how these have been brought about by Fairbairn's theoretical work, and a discussion of the relation between Fairbairn and Freud and between Fairbairn and Melanie Klein; and finally comes a fairly long section on theory and therapy. The author expounds his theme well and brings out comparisons and contrasts lucidly. The quality of the argumentation, however, is uneven; his central arguments are not telling. He did not make quite full use of his opportunity as he might have done if he had sharpened his methodological tools. The volume will be a useful source-book for explanations of a number of points, but not a source-book of cogent arguments on basic issues. But it would be unreasonable to ask for everything in the first work of this kind, for it constitutes the best elementary book in existence on psycho-analysis, and it should even stimulate psycho-analysts who wish to plunge into theoretical problems.

About the section on the sociological watering down of the original theory I propose to say very little. It is not that the people who have done so have had no point; they have had some good points and they have even had some justification when confronted with certain sorts of practical therapeutic problems. But from the point of view of the theory, that is to say with explanation in the most fundamental sense, we must focus on processes that are internal to the mind, for, whatever influence the environment or different environments may have, psychology as such must always be concerned with the impact that that environment makes and how the mind reacts to it. To take an analogy, in 1665 when the Great Plague broke out, no doubt the most important thing was to try to get rid of rats and fleas, as this would afford the most effective way of clearing up the epidemic. But the scientific problem is to understand the various physiological pro-esses that the flea sets up in the body.

I will now try to set out the main theme that the book develops.

Dr Guntrip aims at expounding Freud in such a way as to bring out ply a dichotomy that is surely present in Freud's thinking between ure-seeking aims on the one hand and on the other a totally different ncy of the person, which is to seek relationships with other persons or of other persons. The latter is technically known as object-relation- The two tendencies therefore have to do with drives that are pleasure-g and feelings that are object-seeking. Both are in Freud.

ne of Freud's theories, which he held consistently no matter what other tical modifications he made, was that there is a drive that assumes the of libido, which is stored in the id and seeks the easiest channel of at it can find in order to become discharged and yield maximum e. Libido obeys the pleasure-principle. The person is endowed a certain quantity of libido, and if the ego and super-ego do something to impede the free flow of libido there may be sublimation, so that libido will then attach itself not to directly sexual satisfactions but to non-sexual goals. On the other hand pressure from these sources may produce not sublimations but neurosis and symptoms. Further, libido flows from its source in the id to various sinks which are three bodily zones, namely the oral, anal, and phallic. Libido seeks satisfaction by being discharged in these zones. It will be seen that this is not an object-relational theory.

But Freud was also the celebrated inventor of the Œdipus complex which has to do with the libidinal attachment of a boy to his mother and aggressive fear and hate for his father; this theory (with many others) is object-relational.

Somehow or other the two theories have to be linked: the impersonal libido had somehow to do more than simply bathe a bodily zone; at some stage in early childhood it had to break through the barriers of the zone and attach itself to an object. For this process a theory is lacking. Dr Guntrip objects to the impersonal theory, not because Freud gave no theory providing the linkage required, but for other reasons.

The important discoveries achieved by Melanie Klein centred on emphasising the object-relational aspect of Freud's work. Her work has the look of being far more object-relational. There is a difficulty here in bringing out the situation exactly. Everything in Freud's case histories is object-relational because everything that takes place between patient and analyst must be of that nature; so for this reason it would look as if Freud's work in practice must have been as fully object-relational as Melanie Klein's. The difference lies in the extent to which Freud relies on non-object-relational ideas for explanatory purposes. This he did very heavily, for the libido theory underlay all his work. Although by contrast Melanie Klein's theorising is strongly infused with object-relational ideas, it is not exclusively so.

Guntrip criticises her with justice for not reassessing the non-obje relational parts of psycho-analytic theory, because they would seem to b need of revision in the light of her work. Freud could conceivably seen the need for a similar revision but this need was not nearly so stro in evidence in his time.

It was Fairbairn, in a series of papers from 1940 onwards, who br out the need for this revision both in Freud and in Melanie Klein an attempted to carry it out.

Perhaps one of the most important features of the present book detailed attention that is given to Fairbairn's work, for Fairbairn was thing of a pioneer who, although he worked in Edinburgh com isolated from other analysts, nonetheless through his papers put f revolutionary ideas, some of which have since been absorbed. Fur writes with style, lucidity, and point, and whether he is right or wrong is a significance about his writing which is lacking in most other recent theoretical writings in this field.

Fairbairn came to the conclusion that all human action is object-relational and that the id-libido theory of Freud seeking its own objectless satisfaction is false; that is to say, it is not a case of pleasure-seeking but of object-seeking. This led him to recast the entire structural or topographical theory of the mind and also to revise the theory of psychopathology. A decisive factor in bringing this about was his experience of the overwhelming importance of schizoid processes in all human development both normal and abnormal. He was indeed the first to draw attention to this, though it must be presumed that Melanie Klein was aware of them.

It would not be helpful to describe exactly the nature of the psychic structure that Fairbairn presents. The aim, however, can be brought out simply. It consisted in replacing the id of Freud's theory by another structure which preserves the libidinal aspect, but forms part of the ego which is object-seeking. Fairbairn also replaces destructiveness by another part of the ego that opposes libidinal satisfactions (this is more nearly identical with Freud's super-ego). Further, these various aspects of the ego get split off from one another by a schizoid process, and this he regards as the mechanism underlying the classical mechanism of repression. Fairbairn uses his theory to explain mental diseases falling within the usual categories. His theory might appear to have no consequential difference, but in fact it has. Thus, according to the classical theory, the genital level of libido had to be satisfactorily attained, otherwise object-relationships could not mature satisfactorily. On Fairbairn's theory, however, it would be the other way round, namely unless object-relationships developed satisfactorily the genital level could not be attained. This has immediate significance for therapy and is a deep-seated theoretical idea of considerable interest.

It will be seen that Fairbairn's contribution is by no means negligible. e drew attention to the importance of schizoid splitting, though his hand- g of this is not so developed as that of Melanie Klein. He probably made ecisive contribution in his ideas about ego-structure, and careful examina- might well reveal that he was right in rejecting the classical zone- othesis and the original form of the id-libido hypothesis (though it is ble that something of a similar sort might still play a useful role).

Guntrip does well to bring all this to the fore because Fairbairn's is not so well known as it ought to be, and he also goes to some trouble ke a spirited defence of Fairbairn against some criticisms which have ased on misunderstandings or confusions about methodology—thus have objected to Fairbairn's rejection of the classical id-libido theory t acknowledging the problem, which he rightly unearthed, that led reject that theory. (One should also add that neither Fairbairn nor untrip show an appreciation of the problems confronting Freud that led him, even if wrongly, to form the id-libido hypothesis.) Balanced though Guntrip is among all these uncharted seas, he does, I think, make the occasional slip; for instance, he misunderstands Balint's criticism of Fairbairn, which is reasonable, not fundamentally harmful to Fairbairn's position, and in fact had a sympathetic root. In effect it was that he would sympathise with Fairbairn's point of view personally but thought that it needed more proof. This is certainly reasonable because Fairbairn offered nothing by way of proof, and Guntrip must have sensed this because he goes to great lengths to argue the position in a tone that at least concedes the difficulty of doing so, especially as his argument does not culminate in anything decisive.

This brings me to the main defects or deficiencies of the book. An elementary level of argumentation is good enough for putting misunderstandings straight and getting things into perspective, but it is not enough for handling theoretical issues. Fairbairn is important and should be more widely appreciated or at least studied than he is; it is good, therefore, that Dr Guntrip has put a case for this. But it is one thing to stress the factors that make Fairbairn interesting or important; it is another to show he is right. He may very well be right, but there is no way of deciding so far, and Guntrip has offered no solid help.

Another deficiency is that in presenting something like Fairbairn's views, which are not very well known, although Dr Guntrip presents them on the whole very well, he does not make use of the opportunity to bring out certain decisive points; for instance, Fairbairn invented an idea which he called the ' schizoid position '. Melanie Klein developed what she first called the ' paranoid position ', but in acknowledgment of Fairbairn's work included something of his terminology and changed the name to that of the ' paranoid-schizoid position '. Now there are similarities between the two theories, but there are also differences. It is true that Guntrip doe stress that they *are* different, but he does not go at all carefully into what th differences are. This may appear to be a minor criticism, to do with on one point in an entire book, but it concerns one of Fairbairn's major id concerns a fundamental theoretical issue, and therefore merited deta examination.

The main doubt I have concerns the exegesis of Freud. Dr Guntrip enormous emphasis on the biological and mechanistic aspect of Fr thinking, so much so that one gets the impression of Freud not as th standing psychologist who really did take mental disorder out of the of physiology and into the realm of pure psychology but as one wh still bogged down by physiological thinking. In the light of F psychological results this would be fantastic. It is true that Freud star neurologist and flirted with the idea that one day we might be able to all matters of the mind by chemical means, but at no time in his work use this idea seriously. Dr Guntrip cannot, of coure, have meant any so extreme and unrealistic. But he almost certainly intended to stress the physiological factor in Freud's thinking and to claim that this factor tarnished his results. Even so, the physiological influence seems to be seriously overweighted.

This seems to me a major blemish in Dr Guntrip's work, and interestingly enough there seems to be a methodological confusion that has contributed towards it. He considers that Frued's physiological outlook underlay his pre-occupation with instinct theory. Now in the first place Freud spoke as a rule of drives rather than instincts, and was much more concerned with this idea in a psychological rather than in a physiological sense. So far as Freud was interested in the physiological aspect it was simply that he thought that libido was a psychical representation or a psychical manifestation of physiological changes and urges. This is natural enough and might well be true, but it does not render his psychology physiological. The methodological nature of this mistake now opens out: Guntrip apparently interprets Freud's thinking as physical or physiological just because Freud used physics for constructing models for his thinking. Freud undoubtedly used models from electrostatics, electrodynamics, hydrostatics, and hydrodynamics. But it is a perfectly normal scientific procedure to construct a model from any part of science that one wishes. It is only the formal structure that is carried over from the model to some other field, and such a procedure does not mean that the particular nature of the original content is carried over also. It might well be that the behaviour of persons satisfies hydrodynamic or deterministic principles. This may be wrong, but if it is wrong it is not because of the kind of principle involved. Thus it has been found that there is a considerable similarity in structure between electrodynamics and hydrodynamics, but this does not mean that electricity is in any way watery or
hat water is in any way electrical. In short, it is quite legitimate to import a
hysical model into psychology. Now Dr Guntrip may not have been so
uch concerned with the importation of physical models, though it is hard
escape the impression that he looks askance at the procedure, as with the
icular models that Freud actually did import. For Dr Guntrip depicts a
yton-Helmholtz materialism, that is to say a mechanistic set-up begotten
Newton with further offspring, the physicalistic psychology of Helm-
, and this was the only form of natural science available to Freud to
on. Admitting this, two comments may be made: first that Freud
esponsible for using what he took from physics, and we cannot blame
s if it was not quite what psychology wanted; and second, while
models may be inappropriate, their appropriateness depends on whether
e put to fruitful use or not. Hence Freud's theory has to be judged
own merits and not on the bricks he built it with. No doubt it is
ing to argue, as Dr Guntrip in effect does argue, that the psychology
nal relationship cannot be constructed with ingredients that are

impersonal. But this can be seen to be fallacious. The physics of matter is constructed with ingredients, such as electrons, that are non-material; likewise personal relationship may well contain non-personal ingredients.

It is possible that Dr Guntrip has condensed two criticisms of Freud into one. He portrays Freud as one who failed to emancipate himself wholly from physiological psychology and move into the realm of psychology proper. This in itself is a straightforward misinterpretation, for which there are minor justifications. But Dr Guntrip, I think, has reached this construction, not by studying Freud, for Dr Guntrip is far too well versed in this field to make a mistake like this if the issue were only one of scholarship, but by looking at everything through the eyes of Fairbairn's theory seen as a necessary truth. As I pointed out above, Freud's work was very full of object-relational considerations, both as phenomena to be explained and as explanations (recall how much he sought to explain by means of the object-relational Œdipus complex); and this is pure psychology. At the same time, however, he sought to explain many phenomena by non-objectional considerations. This, I think, is what leads Dr Guntrip to portray Freud as a physiological psychologist rather than a psychologist proper. Now, no-one has shown the impossibility of non-object-relational factors entering into the explanation of object-relations, but even if someone should do this, it would show only that Freud's theory was wrong, not that it was wrong-headed or non-psychological.

I would add however that, embedded in all this Dr Guntrip may have touched on something subtly true. But if there was something about personhood that Freud failed to appreciate, because of current scientific outlook, this would have to be brought out in an altogether different way, without giving a misleading picture of the way Freud's work actuall
presents itself.

One other issue that engenders some irrelevant dispute should perha
be discussed. It concerns the role of the environment and its influence
basic mental processes. It would seem from Fairbairn's writing tha
thinks he lays more emphasis on the environment than does for inst
Freud or Melanie Klein. He does not make much ado about this, bu
Guntrip does, and so do others.

There is quite a widespread idea that Melanie Klein's theory—pe
also her practice—takes too little account, or none at all, of the role
environment in the development, normal or pathological, of the lif
person. Even a moderately careful perusal of her writings makes i
that this is not the case, insofar as she frequently discusses the relatio
person to external objects as well as to internal ones. But perhaps t
point is that, whatever the environment does, it must do it to a m
has a structure and processes, a capacity for becoming patholog
capacity to react, and it is what happens insid

his internal processes so as to handle the trying boss in one or other of a variety of new ways. (We need not now discuss for what sort of case this would be inappropriate. Just as in 1665 it would have been inappropriate to study the pathology of the plague and more fitting to concentrate on hygiene, so with a patient. But, the question under discussion concerns not what form of therapy is to be prescribed, but the different sort of weight to be attached to internal and to environmental factors in explaining a mental state.)

If it is fully realised that psychology and psycho-analysis is only a component of what might explain an individual's state, this issue about the environment might be seen to be vacuous.

To conclude: apart from some minor blemishes and a doubtful reading put on one aspect of Freud's thinking, Dr Guntrip has produced a much needed work in a field where no such work exists, constructed with great care and lucidity, and a work that should do much to promote more accurate knowledge, both among general readers and among psycho-analysts themselves of the various psycho-analytical theories.

J. O. Wisdom

The Physical World of Late Antiquity. By S. Sambursky.
Routledge and Kegan Paul, London, 1962. Pp. xii+189. 21s.

s is the third volume in a series of books (see this *Journal*, 1958, **8**, 347
1961, **11**, 262) in which Professor Sambursky has described the develop-
of Greek physics. The present book covers the period from the death
ristotle to the sixth century A.D.

he first chapter deals with the concepts of space and time. Immedi-
after Aristotle, the opposition between *absolute* and *relative* concepts was
ning to show. It occurred when Aristotle's static ' place ' was changed
e Stoic *pneuma* into a dynamic space in which all things mutually
t. This is the successor concept to the Atomists' void in which
es collide with one another. Theophrastus emphasised the order in which bodies are placed and so attributed a relational, and therefore relative, character to space. Strato took space as a box to be filled with bodies and so his conception is nearer to that of absolute space. Iamblichus pointed

out that space depends on cause when it becomes dynamic. Proclus, the Neo-Platonic, identified space with light; his model is a luminous sphere. All these conceptions contributed to the aether theory of the nineteenth century.

The corresponding concepts of time are more difficult to grasp. The Greeks never had a clear definition of either velocity or acceleration. Aristotle said that we measure time by motion; this was not very helpful since the meaning of ' motion ' was unclear. Moreover, he said also that we measure motion by time; and this physical reciprocity engenders a logical circularity. The difficulty stems mainly from the Greek preference for circular movement as something indestructible. Linear time is not easily isolated from periodic processes. Thus the puzzles of the *specious present* and of the *second time*, or ' time of time ', were begotten that have fascinated philosophers ever since.

The history of the mechanistic theory of matter is given in the second chapter. The Atomists omitted force as a separate entity from their theory; while the Stoics identified it with stress in the *pneuma*. Plato's geometrical conception of matter, though in some sense atomic since concerned with shape and form, stems from Pythagoras. When it is said that Platonism has come back into modern physics, it is of course the basic Pythagorean invention of number as pattern that has enabled us to mathematise our theories. Aristotle opposed to it his ' qualitative ' theory; he explained change by the combination of the qualities of the four basic elements. This theory as well as his maxim, ' perceptible things require perceptible principles ', prevented Aristotle from constructing an adequate system of physics. Iamblichus, though being influenced by oriental mysticism, rebutted Aristotle in an almost modern manner because he went back to Pythagoras for his argument.

The next chapters treat of sublunar and celestial mechanics and physical action. The impetus theory is contained, *in nuce*, in Aristot work when he looks for a mechanism of transmission of motive po Hipparchus, two hundred years later, first overtly introduced the con of impetus. Philoponus speaks of two kinds of impetus to which he the Aristotelian names of *dynamis* and *energeia*. The theory became and more complicated and of doubtful value therefore.

Celestial physics has left its mark on our theories. This is noticea the ideal of a ' predictive ' explanation. The theory of eccentric cycl of epicycles was designed to ' save the phenomena '. Proclus threw on it; he wanted a causal rather than a geometrical explanation. Church attacked the theories of celestial physics simply because they to get rid of Greek paganism, that is, of the scientific attitude associated with it. Philoponus dismissed the Ptolemaic system as ' entirely divorced from reality '.

The great problem was, How is action transmitted through space? Aristotle emphasised the contiguity of the acting body and the body acted upon. The Stoics used the spider's web as a model for transmission. Slowly philosophical principles make their appearance when the nature of physical action is discussed. 'Nature does nothing in vain', etc. The action principles of Fermat and Maupertuis derive from this origin.

The book ends with an account of the unity of heaven and earth that was gradually being achieved against the Aristotelian dichotomy between them. Two distinct signs of the coming renaissance of scientific thought can be seen at the close of antiquity in the sixth century A.D. Philoponus anticipated the breakdown of the barrier between heaven and earth, while Iamblichus, Proclus, and Simplicius recognised mathematics as the symbolism necessary for the successful description of nature.

The history of science is not the mere succession of discoveries and inventions: it must be seen as the natural evolution of ideas. Our account of it cannot be restricted, therefore, to intellectual and conscious thought processes alone. Unconscious processes must be considered to play their part in the growth of knowledge as well; and, indeed, the phantasies that are so abundantly expressed by early scientists show up the irrational background of rational thought. Professor Sambursky tends to reject this kind of evidence—what he calls mysticism and obscurantism—for explaining how scientific ideas have evolved. Though I personally prefer a more complete account (including, for instance, the social development), I am very glad to have this straightforward history of a very neglected period.

ERNEST H. HUTTEN

Time and the Physical World. By Richard Schlegel.
Michigan State University Press, 1961. Pp. xii + 211. $7.50

E present book, by a physics professor at the Michigan State University,
ents many sides to the question of time and, indeed, in an admirably
fashion. The author advocates the relational doctrine of time, i.e.
Aristotle-Lucretius idea that time, far from flowing of itself, is insepar-
from events and is a sort of pace of change. This idea—the historical
of which are barely mentioned—is adequately presented in Chapter I,
Time and Physical Process', which is probably the best. The remaining
ers provide additional arguments in favour of the doctrine. On more
tal issues the book is disappointing.

ncept of time appears so crystal-clear to some, and so obscure to others, that very few studies have been devoted to it. A careful analysis of this concept is supposed to require both factual science and logic; unfortunately, we are still waiting for a monograph on time meeting these requirements, and for a long while we may at most hope for stimulating essays on that elusive yet basic concept. Logicians have tended to focus on sentences containing the term ' time ' without enquiring into either the logical structure of the concept or of the theories involving time, and physicists have tended to overlook the logical problems of time as well as the peculiarities of non-physical (i.e. psychological, biological, and historical) time. As a result, although we have physical theories of time or, rather, of space-time (namely, relativity theories), we lack a philosophical theory of time. The present book, however, fails to fill this gap.

The reading of the book requires only elementary physics and no philosophical background at all. Only the realisation of the book's inadequacies requires a little more. In the first place, logic is not handled by the author as a tool of analysis. Consequently we are not told why, or at least that, time is usually regarded as a primitive concept in physics, and nothing is said of the structure of the time concept, although it should not be impossible to say a few things about this, if only to lift the mist that envelops the time concept in philosophy. For example, in the context of the relational doctrine of time one might take duration as a primitive and regard it as a two-place numerical functor, namely thus: ' the duration, in seconds, of the event [or process, or cycle] *x*, relative to the reference system *y*, equals the number *t* '. Then, one might define ' instant ' as the frontier between any two contiguous non-overlapping durations, and ' time ' as the set of all durations.

In the second place, the old philosophical problem of the reality of time is not approached, presumably on the assumption that, since material change —which is assumed to be real—is described with the help of the time concept, then the latter must have a real referent. Yet not all physical variables need symbolise real traits. Besides, there is a simple and common device by means of which time can be ' eliminated '. In fact, ' *x* occurred at *t* relativ[illegible] to *y* ' can always be replaced by ' *x* occurred jointly with *z* relative to *y*[illegible] where ' jointly with ' may, but need not, be defined as ' at the same time a[illegible] After all, timing by means of clocks is precisely such a device for establish[illegible] one-many correspondences among sets of events. From the proposi[illegible] that, if there is no change, then there is no time, we infer (by contraposi[illegible] that, if there is time, then there is change. But we cannot infer that ti[illegible] in the things themselves, just as from the applicability of arithmetics w[illegible] not infer that numbers have an independent existence. A care for [illegible] would have made such an analysis possible and would have preclud[illegible] occurrence of inaccurate phrases such as these: ' the essential elements of the entropy concept ' (p. 21), ' a force acting on a mass ' (p. 32), or ' the uncertainty relation prevents a physical process from being without time error ' (p. 168).

Schlegel's discussion of physical theory could often be improved. Thus, of light one is at liberty to do it, but one will not succeed in this enterprise by re-interpreting theorems of a theory of mechanics as propositions regarding photons—let alone by introducing an anthropomorphic and finalistic conjecture. Knowledge about photons is preferable to assumptions regarding knowing photons.

In conclusion, this book is one more failure in the age-old attempt to grasp the concept of time. On this occasion, the source of the failure is clear: want of logic and sloppy science. The relational theory of time surely deserves a further, more careful, treatment.

MARIO BUNGE

Thinking and Perceiving. By John W. Yolton.
Open Court, Illinois, 1962. Pp. xiv+161. $3.50.

PROFESSOR YOLTON'S book has four main themes: (1) That at least in the sphere of perception and thought philosophical theory can validly interpret the scientific (especially the psychological) evidence in its own way, and that the evidence throws light on philosophical problems. The whole book is meant to illustrate this, and Yolton supports it by the theoretical position, developed also in this *Journal* in November 1959, that there is a generic 'systemic' type of explanation of which explanations in science and philosophy are species (Chapter 6). A systemic explanation represents the explicanda in a conceptual scheme, placing them in the context of presupposed ideas. (So far as philosophy of science is concerned this adopts the approach of Conant, Toulmin, and Hanson.) The conceptual schemes in philosophy may, he claims, be independent of science with their own alidity. The difficulty then lies in ensuring that the schemes and preppositions are not just arbitrary, and in deciding between rivals. As perimental checks are not available, philosophical explanations must it ns be controlled by 'principles of order' and 'intelligibility'; but these [illegible] vague terms and in his analysis Yolton finally has to appeal to prior ferences' and 'commitments' (pp. 146 ff.). It is difficult to see how [illegible] can be anything but arbitrary—indeed his own ontological speculations eem to many to show the lack of precise objective controls. One may agree that philosophical theories have an independent value; but it is [illegible] plausible to suppose that this lies in the conceptual analysis and poten- fruitful innovations they contain, or in their synoptic approach uniting fic, introspective, historical, and other evidence.

[illegible] That there is an essential continuity between thinking and other nses of the human organism to its environment. Thinking has in fact emerged from them—'we have a ladder-like analysis where each later step builds upon its predecessor' p. 113—and the steps are traced from the earliest physiological reactions and accommodations of the baby, up through dawning recognition, through fully developed perception, and so to abstract thought (Chapters 1, 2 and 4). Instead of a direct assessment of the evidence, Yolton's method is to summarise and compare what a whole range of thinkers have said on the subject, laying particular stress on their views about the essential point of changeover from physiological or behavioural responses to conscious ones. This gives a useful survey of opinion, mostly psychological and pragmatist, and shows a family resemblance between many different theories. But it is a disconcerting method in which the plan is not immediately apparent; the bewildered reader may feel he is just being presented with a catalogue of second-hand material. This is to a fair extent redeemed by Yolton's illuminating critical discussion (pp. 77 ff.) of (*a*) the relation between neuro-physiological mechanisms and the conscious process of recognition, and (*b*) the problems involved in the analysis of the latter. But better organisation and sign-posting of the earlier chapters is required, and more direct evidence.

(3) Interwoven with the general exposition of the ' ladder ' from physiological responses to thought is a polemic against those who have supposed that a neurological, behaviouristic, linguistic, or dispositional analysis of mental processes is adequate. Main targets are Hayek for the neurological view, and Mead, Merleau-Ponty, Ryle, and Wittgenstein (the last two especially) for attempting to externalise mental phenomena and above all for regarding mind and thought as constituted by the evidence for them. This is a useful discussion not only for the criticisms but also for the way it shows affinities between different thinkers whose work may at first sight seem unrelated or even isolated.

(4) Besides arguing that thought differs from but nevertheless emerges
out of precognitive activities, Yolton attempts a positive ontological theor
of it. Following the Würzburg school he maintains the existence of imag
less thought, but goes further in claiming that this thought is the awaren
of mental entities (' concepts ' or ' meanings '). In his view empiri
philosophers have refused to accept this because they think that such ent
must be introspectible or scientifically detectable; but awareness of m
entities is not a sort of looking or seeing—it is a mental grasp or underst
ing of meanings, it is a form, at a more abstract level, of recognition o
sponse to meaningful stimuli (p. 104). Finally he arrives at a ' conce
phenomenalism ' in which concepts are regarded not only as ' units o
nificance ' but also as irreducible ' units of reality '. Few empiri
minded philosophers will be in sympathy with this, but they should not
their suspicion of these mysterious entities or their dismay at the reintr
tion of hypostatised ' meanings ' to condemn unread the valuable exte
criticism of Price and others through which Yolton climbs (or descends) to
his metaphysics. He makes out a good case for his view that dispositional

theories do not adequately account for our understanding of meanings and symbols or for imageless thought. This does not mean, however, that one must follow him in postulating a range of obscure mental entities; the deficiencies might perhaps be met by extending Price's notion of the sub-activation of concepts, by allowing inchoate symbols, or by a better analysis of 'understanding'. But at any rate those who find Yolton's cure worse than the disease must now search around for a better one, for he has at least shown that the current philosophical theories of thinking are far from healthy.

R. J. Hirst

Aristotle's System of the Physical World. A Comparison with his Predecessors.
By Friedrich Solmsen.
Cornell University Press, Ithaca, New York, 1960. Pp. xiii + 468, 60s.

The author describes in detail the physical theories of Aristotle. After an historical introduction, the main part of the book is devoted to the Physics; then follow chapters on Cosmology, on the Genesis of physical objects, and on Meteorology. It is a very complete study of Aristotle's writings.

Aristotle was a biologist before he became a philosopher. He began teaching in the Lyceum only after he had spent some ten years in observing animals and collecting specimens. This early interest marks his philosophy; and it is the main reason why his physics is not successful. The biological approach is too 'concrete' and observational, while 'abstraction' and speculation are needed for inventing physical theories, as is demonstrated y the success of the Ionian cosmologists. Aristotle's lack of sympathy for e mathematical approach of the Atomists is part of the same emotional itude.

Change and movement were the great problems of the Greeks. Aristotle [illegible] it in terms of Becoming and Perishing, *genesis* and *olethros*, birth and h—concepts that Parmenides already had vetoed. The 'dreaded con- of not-being' thus is re-introduced by Aristotle. The Atomists had come the difficulty by explaining visible changes as the combination eparation of indestructible, ceaselessly moving, particles which, how- have to be invisible. Aristotle's physics represents a regression when pared to atomism. His theory of natural movement is based upon the of a 'natural place' and of a 'cyclical time'. The 'eternal' movement cular, unlike the straight-line motions of the atoms of Democritus and ucippus or of Galileo's masspoints. This 'natural' movement is perfect since complete in itself; and its perfection derives from the medium in which it takes place, the aether. The aether—the fifth element which

Aristotle renames the first body—is ' unageing ' and ' not suffering ' any change. It is directly visible, cyclical movement of the heavens that is taken as the model. Let us remember here, by the way, that the aether of the last century, too, has the property of allowing light to pass through without change, i.e. absorption ; and so we can ascribe to light an ' ideal ' phase velocity.

Even for the eternal movement there exists an ' unmoved mover '. Aristotle was unable to accept the idea of a truly ceaseless motion that, when explained by Galileo in terms of inertia, became the basis of mechanics and hence of modern physics. The denial of a Prime Mover was essential for the foundation of science. In Aristotle's scheme there are fifty-six of them in all. This is an example illustrating how false it is to interpret Aristotle's thought in terms of the Jewish-Christian religion. Aristotle, even more than Plato, has suffered from being taken as the prophet of a religion that was founded three hundred years after his death and whose advocates used him, a good thousand years later, to make eastern mysticism acceptable to the western mind. That this is quite alien to Aristotle is shown, too, by the fact that metaphysics, ' the science of true being must not sets limit or give directions; still less can it provide eternal and perfect prototypes to which physical objects and phenomena would relate as do shadows to the true realities '. Aristotle kept physics strictly apart from metaphysics.

This is an excellent book, a mine of careful, classical scholarship, that yields valuable ore to the historian and philosopher of science.

ERNEST H. HUTTEN

The Logic of the Humanities. By Ernst Cassirer. Trans. by Clarenc
South Howe.
Yale University Press, New Haven, 1961. Pp. xx+217. $4.50.

THE title of the English translation of this book is either misleading nonsensical, since it is difficult to understand in what sense the human can have a logic. But this should not, in itself, deter any prospective re provided he knows what he is really in for. The book is in fact abou ' cultural sciences ' and the enquiry has little or nothing to do with lo understood by most readers of this journal. The main theme of the is this: the cultural sciences (Kultuurwissenschaften) seek to interpret sets of symbols of which human culture—language, myth, religion, consists.

Do the cultural sciences have anything in common with the sciences? Superficially, it would seem that they do not, since the fo are concerned with form and the latter with causality. But there is contradiction between these two concerns, for the natural sciences have terminology of ‘ collective ’ and ‘ place selection ’, it is not a long step from his work to that of von Mises. For he realised already that the theory he offered involved idealisation (p. 163) and inductive generalisation from the statistical records of experience (p. 233). What is especially interesting to a modern reader is his rejection of the whole fabric of probability theory elaborated by Bernoulli and Laplace. He realised, of course, that some of their mathematical results could be retained as theorems about combinations, but he refused to allow that these had any bearing on probability as he understood it, and his objection was not just that Bernoulli and Laplace tried to make illegitimate transitions from degrees of confidence to frequencies and back again, but rather that they talked in an unintelligible way about objective probabilities:

> If one might judge by the interpretation and uses to which this theorem [of Bernoulli] is sometimes exposed, we should regard it as one of the last remaining relics of Realism, which after being banished elsewhere still manages to linger in the remote province of Probability. . . . A uniformity is observed; sometimes, as in games of chance, it is to be so connected with the physical constitution of the bodies concerned as to be capable of being inferred beforehand; though even here the connexion is by no means so necessary as is commonly supposed. . . . This constitution is then converted into an ‘ objective probability ’ supposed to develop into the sequence which exhibits the uniformity (pp. 91-92).

This passage has a significant similarity of tone to a Humean attack on talk of necessary connection, and it seems to me to show very clearly that Venn thought of his own theory as reductivist, i.e. as a theory for the elimination of all attempts to *explain* uniformity in the phenomena. But such theories are apt to produce their own nemesis. On his title page Venn printed as a motto for the whole book Tennyson's lines about Nature:

So careful of the type she seems
So careless of the single life.

No doubt he did not wish this personification of nature to be taken seriousl but the quotation is pointless if it is not intended to suggest that the ser which interest him are directly subject to *laws of chance*, and that is a th which I personally find more difficult to accept than the old talk of objec probability. Incidentally it is a thesis involved in the orthodox interp tion of quantum theory.

WILLIAM KNEA

Imagination. By E. J. Furlong.
George Allen and Unwin, 1961. Pp. 125. 18s.

PROFESSOR FURLONG distinguishes three main usages of the term ‘ im tion ’. A child may be playing bears *in imagination* (he is lying in h day-dreaming); he may be playing bears *with imagination* (his ma themselves become increasingly concerned with form—that is, with wholes, which are in fact patterns or structures of relationships; furthermore, the examination of cultural forms itself leads to a causal enquiry into the ‘ mental *processes* from which they have come into being and whose product they are ’ (p. 174). However, there is no possibility of explaining culture in non-cultural terms—that is, of reducting culture to nature—for, although culture is the mirror in which we recognise the image of humanity, there is no looking behind it.

While the discussion of form can be seen to constitute the main substance of this book, there is no serious attempt to tackle one of the most interesting problems of the philosophy of science, namely, the logic of explanation and analysis of social and cultural ‘ wholes ’. In fact, this book is not really concerned with the logic of anything. It is a very scholarly attempt to philosophise culture, and those who have an appetite for this sort of thing will no doubt devour the hidden morsels which lie concealed behind the verbal symbols. This reviewer found these morsels rather like some Chinese meals: aesthetically pleasing at the time, but unsatisfying a little later.

PERCY S. COHEN

The Logic of Chance (Fourth edition). By John Venn.
Chelsea Publishing Company, New York, 1962. Pp. xxix+508.
Cloth bound $4.95; paper back $2.25

THIS is an unaltered reprint of the third edition, which appeared in 1888.
On the cover of the paper back the publishers describe it correctly as ‘ one
of the classics of the theory of probability ’ and go on to say that it ‘ remains
nsurpassed for clarity, readability, and sheer charm of exposition ’. I think
would be going too far to say that it deserves to be read by beginners in
bability theory with the attention that students of induction are still
ed to give to Hume and Mill, but it certainly deserves study by all
osophers who wish to understand the motives that have led many
ticians (perhaps most) to say they believe in a frequency theory of
bility.

nn was not the first person to suggest that the probability of an A
s being B should be defined as the limit towards which the relative
ency of B things tends in an indefinitely continued series of A things.
nself notices the similarity of his own view to that of Leslie Ellis. But
s the first to try to give precision to talk about ‘ the long run ’ and
nness, and the first to base his whole theory on the explicit assumption
e can find in nature certain series which combine ‘ aggregate order ’
individual irregularity ’ (pp. 9 and 96). Although he did not use the

playing shows imagination); or he may be *imagining* he is a bear (his mother has said to him 'Imagine you are a bear'). Furlong calls the child's imagining he is a bear 'plain supposal' to distinguish it from the case in which the child, frightened by the dark, falsely believes there really are bears around ('false supposal'). Apart from chapters on these different usages of 'imagination' there are chapters on 'Dreams—what they are' and 'Dreams—what they prove', on 'Images', on 'Art and Imagination', on 'Hume's treatment of imagination', and on 'Imagination in Kant'. The book is, in fact, more a collection of essays on different topics all of which involve the concept (or concepts) of imagination, than an exposition of a single philosophical thesis. The longest chapters are the ones on 'Dreams—what they are' and on 'Hume's treatment of imagination'. I shall comment briefly on the former.

Furlong's reason for including chapters on dreaming is that common speech suggests that when we are dreaming, we are imagining, in some sense. Furlong himself holds most dreams to be instances of 'false supposal'. This brings him into conflict with Professor Malcolm's thesis that 'it is nonsensical to suppose that while a person is asleep he could make any judgment' (*Dreaming*, p. 36). This thesis has therefore to be shown to be unacceptable. Malcolm's argument, Furlong says, is that it is senseless to talk of experiences occurring while we are asleep because the occurrence of such experiences could not be verified either by those observing the sleeper, or by the sleeper himself. Furlong's answer to this is that in so far as a person's memory counts as evidence for a waking event so also, unless some good reason to the contrary is forthcoming, it counts as evidence for the occurrence of sleeping events; and hence Malcolm's verification requirement is supplied. Malcolm might reply that whereas memories of waking events can be checked, memories of dreaming events cannot. But there are many memories of non-dreaming events (e.g. memories of what one was thinking) hich are not checkable. And yet 'distinct recall' would usually be arded as an adequate verification of such events. So the lack of in-endent verification cannot be consistently used as an argument against pting as evidential such recollections as 'I had a nightmare last night'. What Malcolm would say in answer to this I do not know. He might t it was never his intention to deny that it might seem to a person rtain things had happened which had not in fact happened, and that sense the person might be said to 'remember' what he had in fact ed. Malcolm might go further and admit that a person might that he judged he was dreaming (for what limits can there be to what ay dream?). It might be that the only thing Malcolm would deny is it is meaningful to speak of a person dreaming and, at the same time, dging (i.e. not merely dreaming that he is judging) that he is dreaming.

G. N. A. Vesey

ANNUAL CONFERENCE OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

THE Seventh Annual Conference of the British Society for the Philosophy of Science was held from 21st to 23rd September 1962, at Tetley Hall, Leeds University. It comprised a Colloquium of four symposia and was attended by some sixty members and guests. Mr F. T. C. Harris of Middlesex Hospital Medical School was Conference Secretary.

The four symposia were entitled, 'The Idea of a Physical Quantity', 'Fundamental Particles and Identity', 'Kant's Regulative Principles and their application to Science' and finally 'The Nature of Law in Social Science'. In conformity with the Society's usual practice each symposium heard two symposiasts and then entered into a discussion of the papers.

With the Society's President, Professor R. B. Braithwaite in the chair, Dr J. R. Ravetz, Leeds University, introduced the first symposium with a historical paper 'Galileo and Physical Quantities' which discussed the belief in the existence of physical quantities and the problem of assigning measures to these quantities. Dr Ravetz claimed that the first of these was an essential assumption in Galileo's investigations whilst the second is necessary for the construction of a fruitful natural philosophy. There followed an enquiry into the relationship between the concepts of 'quality' and 'quantity' in which the speaker, Dr H. V. Stopes-Roe, Birmingham University, argued that the former concept included the latter. For example, the quality brightness can be given a measure and so construed a quantity. Galileo's idea of fundamental individual quantities was claimed by the speaker to be inadequate, leading Galileo away from the formulation of a coherent approach to units and thus away from numerical results. On the other hand Dr Stopes-Roe suggested that Fourier's approach to this problem similarly failed to treat the unit satisfactorily. He went on to suggest that the concept of quantity should be taken as basic and combined with a liberal approach to the analysis of the concept 'quantity'.

Dr C. W Kilmister, King's College, London, took the chair in the second sym[illegible] posium, the first paper of which was a very thorough survey by Dr H. R. Post, [illegible] the Chelsea College of Advanced Technology, Dr Post explained that element[illegible] particles of one type had no individuality. It was a fallacy to derive this nonindiv[illegible] ality from Heisenberg's Principle. In fact it derived from the nature of the theor[illegible] structure of quantum mechanics. Fundamental particles are without 'seco[illegible] qualities', he insisted, and their lack of individuality derives less from their [illegible] logical status than from their high level of abstraction. In the second pa[illegible] E. W. Bastin, King's College, Cambridge, took the standpoint of ontological [illegible] He argued that in electron spin one had a property which, as such, was u[illegible] identifiable, but must of course be attributed to individual events, particles, [illegible] existed as such. In discussion Professor Braithwaite and Dr Kilmister took t[illegible] of Nature, the latter with currently new empirical data.

Professor S. Körner, Bristol University, was chairman to the third sympo[illegible] which was opened by Mr Jonathan Bennett, Cambridge, with an argum[illegible] directed to disproving Kant's view that a science in which some generalisations have exceptions is intellectually disreputable. Contrary to this, the speaker held that the scientist must qualify his generalisations if he is to avoid confusion. Mr Bennett explained a 'regulative principle' as one that had no other justification than its utility. Thus, standing unanalysed, it bears comparison to a logical axiom. The Kantian view that everything has a cause is just such a principle. Mr Bennett went on to show that Kant's claim that the principle of causation had such a status is in fact at variance with that writer's attempted proof elsewhere, of that very same principle: but under the name 'the second analogy from experience'. Entirely to the contrary, Dr G. Buchdahl, The Whipple Museum, Cambridge, accepted Kant's distinction between a general principle of causation and a 'regulative' principle of determinism. He held that, of the two versions of the deterministic principle discussed by Mr Bennett, only the first is truly regulative and it is this version that permits the law-like nature of empirical generalisation. The Chairman introduced the discussion and drew attention to the philosopher's distinction between logical implication and nominal implication, or implication reflecting things as they are. He pointed out that Kant did not regard Newtonian Physics as a regularity that happened to be observed but did in fact believe it to be true. As Professor Körner remarked, 'The opus of great philosophers is a kind of philosophical Rorschach test'.

The discussion of the final symposium was carried out under the guidance of Mr J. W. N. Watkins, London School of Economics. The symposium opened with a paper by Mr J. G. H. Newfield, also of L.S.E., which criticised the views expressed in Mr P. G. Winch's 'The Idea of a Social Science and its Relation to Philosophy' (London, Routledge and Kegan Paul, 1958). Mr Newfield argued that to criticise sociology by means of *a priori* philosophical criteria was to make a category mistake. For example it was not a prerequisite for sociology that sociologists study the language of their science, a test procedure for laws was simply to distinguish between their valid and invalid applications and this did not necessarily involve formulating the law. Mr P. G. Winch, University College of South Wales, argued that the Humeian position taken by the previous speaker was fully answered by Kant, and it was this philosopher that Mr Winch followed. It was not the case that regularities were first observed and then logic entered the field. It was more cogent to argue that the observation of empirical regularities was only possible within a preconceived ame-work of order, which was an argument in accord with the hypothetic-ductive viewpoint of Professor K. R. Popper. Mr Watkins agreed with the second aker that phenomena arise from language. He also argued that Mr Winch's oach did not admit of social criticism in that it had not been equipped with a language. Professor W. C. Kneale, the President-Elect, took up a point conng the reducibility of sociological concepts to dispositional statements about duals. He pointed out a fundamental difference between the reduction of a oural science, sociology, to psychology and of a physical science, chemistry, sics, in that the former reduction would have to embrace two different conl schemes. For example, it was not possible to describe the spread of economic nption in psychological terms.

D. G. HARRIS
F. T. C. HARRIS

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Received for Review

Allan, D. J., *Aristote, le Philosophe*, Nauwelaerts, Louvain, 1962, pp. 250, FB. 120

Armstrong, D. M., *Bodily Sensations*, Routledge and Kegan Paul, London, 1962, pp. viii + 132, 12s. 6d.

> 'Bodily sensations are a sub-class of *sense*-impression. Standing in perception of our bodily state as visual impressions stand to the sense of sight.' This is discussed.

Beloff, J., *The Existence of Mind*, Macgibbon & Kee, London, 1962, pp. 263, 35s.

Bickel, L., *The Unity of Body and Mind*, Philosophical Library, New York, 1959, pp. 167, $3.75

Bohr, N., *Atomic Theory and the Description of Nature*, Cambridge University Press, 1962, pp. 119, 8s. 6d.

Bridgman, P. W., *The Thermodynamics of Electrical Phenomena in Metals and a Condensed Collection of Thermodynamic Formulas*, Dover Publications, New York, 1962, pp. viii + 224, $1.65

Broad, C. D., *Lectures on Psychical Research*, Routledge and Kegan Paul, London, 1962, pp. xi + 450, 56s.

Calder, R., *Living with the Atom*, University of Chicago Press, 1962, pp. xiv+275, 42s.

Capek, M., *The Philosophical Impact of Contemporary Physics*, D. Van Nostrand Co., New Jersey, 1962, pp. xvii+414, 30s.

Carnap, R., *Der Logische Aufbau der Welt*, Felix Meiner, Hamburg, 1962, pp xx+336, DM.28

Crombie, I. M., *An Examination of Plato's Doctrines, 1. Plato on Man and Society*, Routledge & Kegan Paul, London, 1962, pp. xii+395, 42s.

Crosland, M. P., *Historical Studies in the Language of Chemistry*, Heinemann, London 1962, pp. xviii+406, 50s.

Donagan, A., *The Later Philosophy of R. C. Collingwood*, The Clarendon Pre Oxford, 1962, pp. xi+332, 42s.

Feibleman, J. K., *Biosocial Factors in Mental Illness*, Charles C. Thomas, Illinois, pp. viii+111, $5.00

Feibleman, J. K., *Foundations of Empiricism*, Martinus Nijhoff, The Hague, pp. xi+389, Dfl.27

> An attempt is made to set up a metaphysic 'within the limits of log empiricism'. The theory of induction is discussed. Also discussed relationships between propositions and facts.

Feigl, H. & Maxwell, G. (Eds.), *Philosophy of Science*, Vol. III, University of sota Press, Minneapolis; Oxford University Press, London, 1962, pp. xv 68s.

Feigl, H. & Maxwell, G. (Eds.), *Current Issues in the Philosophy of Science*, H Rinehart & Winston, New York, 1962, pp. xii+484, 48s.

> This is a discussion by well known philosophers of science, most of whom have elaborated their views in more detail elsewhere.

RECENT PUBLICATIONS

Finberg, H. P. R. (Ed.), *Approaches to History*, Routledge and Kegan Paul, London, 1962, pp. x+221, 25s.

Contains one chapter on the History of Science by Professor A. R. Hall.

Foss, M., *Logic and Existence*, Philosophical Library, New York, 1962, pp. vii+240, $4.75

Frisch, O. R., *Atomic Physics Today*, Oliver and Boyd, Edinburgh, 1962, pp. v+254, 25s.

George, F. H., *Cognition*, Methuen & Co., London, 1962, pp. 309, 32s. 6d.

Gödel, K. (trans. B. Meltzer), *On Formally Undecidable Propositions of Principia Mathematica and Related Systems*, Oliver and Boyd, Edinburgh, pp. xviii+72, 12s. 6d.

Good, I. J. (Ed.), *The Scientist Speculates*, Heinemann, London, 1962, pp. xvii+413, 15s.

'An anthology of partly-baked ideas'. Insufficiently baked to be reviewable but fascinating.

Hooke, R., *Micrographia*, Dover Publications, New York; Constable & Co., London, 1962, pp. xliv+273, $2.00

Jelenski, K. A. (Ed.), *History and Hope*, Routledge and Kegan Paul, London, 1962, pp. vi+208, 25s.

Jonas, A. D., *Irritation and Counterirritation*, Vantage Press, New York, 1962, pp. 368, $7.50

Kazemier, B. H. and Vuysue, D. (Eds.), *Logic and Language*, D. Reidel Publishing Co., Holland, 1962, pp. 246, Dfl. 24.50

Kneebone, G. T., *Mathematical Logic and the Foundations of Mathematics*, Van Nostrand, London, 1963, pp. xiv+435, 65s.

Lawrence, D. H. & Festinger, L., *Deterrents and Reinforcement*, Tavistock Publications, London, 1962, pp. x+180, 32s.

Leblanc, H., *Statistical and Inductive Probabilities*, Prentice-Hall Inc., New Jersey, 1962, pp. x+148, $6.65

Lee, H. N. L., *Symbolic Logic*, Routledge & Kegan Paul Ltd., London, 1962, pp. ix+356, 35s.

ondon, Fritz, *Superfluids*, Dover Publications, New York, 1962, pp. xviii+173, $1.45

chei, E. C., *The Logical Systems of Lesniewski*, North-Holland Publishing Co., msterdam, 1962, pp. vii+361, Dfl. 72

en, A. I., *Free Action*, Routledge and Kegan Paul, London, 1961, pp. x + 6, 20s.

This purely philosophical enquiry into the freedom of the will is carried out by examining a number of concepts such as action, bodily movement, desire, motive, and reason for doing.

el, D. H. (Ed.), *Selected Papers on Physical Processes in Ionized Plasmas*, Dover blications, New York, 1962, pp. viii+374, $2.95

sner, R. O., *Die zwei Grunbereiche der Metaphysik*, Hermann Böhlaus, Vienna, 1961, pp. 133, S. 86.

Merleau-Ponty, M., *Phenomenology of Perception*, Routledge and Kegan Paul, London, 1962, pp. xxii+466, 56s.

Nagel, E., Suppes, P. & Tarski, A. (Eds.), *Logic, Methodology and Philosophy of Science*, Stanford University Press; Oxford University Press, London, 1962, pp. x+661, £5

An indispensable volume of papers read at 1960 International Congress for Logic, Method and the Philosophy of Science.

Natanson, M. (Ed.), *Alfred Schutz—Collected Papers I*, Martinus Nijhoff, The Hague, 1962, pp. xxiv+361, Dfl. 28

This posthumous volume of papers contains some comments on the methodology of the social sciences which were first made by Popper some ten years earlier though the author does not seem to have known of Popper. The book is mainly a presentation of phenomenology and includes, for instance, an account of Max Scheler's work, which has never been translated into English.

Nelson, L., *Fortschritte und Rückschritte der Philosophie*, Verlag Offentliches Leben, Frankfurt am Main, 1962, pp. 768, DM. 48

Nidditch, P. H., *The Development of Mathematical Logic*, Routledge and Kegan Paul, London, 1962, pp. viii+88, 7s. 6d.

Ofstad, H., *An Inquiry into the Freedom of Decision*, Norwegian Universities Press, Oslo; George Allen & Unwin Ltd., London, 1962, pp. xvi+391, 42s.

A philosophical analysis of the formulation of 'The person P decided freely in the situation S.' Involved in this is the issue of power. The relation of this to ethical systems is discussed extensively.

Ortman, E. J., *Philosophy of Teaching*, Philosophical Library, New York, 1962, pp. 398, $4.75

Planck, M., *A Survey of Physical Theory*, Dover Publications, New York, 1962, pp. x+117, $1.15

Plochmann, G. K. & Lawson, J. B., *Terms in their Propositional Contexts in Wittgenstein's Tractatus*, Southern Illinois University Press; W. S. Hall, London, 1962, pp. xiv+229, 56s.

Rhys, H. H. (Ed.), *Seventeenth Century Science and the Arts*, Princeton University Press Oxford University Press, London, 1962, pp. viii+137, 24s.

Scholarly essays on Science and Arts, Science and Literature, Science and t Visual Arts, Scientific Empiricism in Musical Thought.

Routh, E. J., *A Treatise on Dynamics of a Particle*, Dover Publications, New Y 1962, pp. xi+417, $2.25

Sinclair, U., *Mental Radio*, Charles C. Thomas, Illinois, 1962, pp. xv+237, $8.

Smelser, N. J., *Theory of Collective Behaviour*, Routledge and Kegan Paul, Lo 1962, pp. xii4+36, 45s.

Sprague, R. K., *Plato's Use of Fallacy*, Routledge and Kegan Paul, Londo pp. xvi+106, 18s.

Themerson, S., *Cardinal Pölätüo*, Gaberbocchus, London, 1961, pp. 184, 15s.

This is brilliant satire—as was the author's novel *Professor Mmaa's Lect* times extraordinarily funny, but makes such plentiful allusion to s philosophy of science, and philosophy that it must remain unknown t intelligentsia so long as our two-culture society lasts.

Thorpe, W. H., *Biology and the Nature of Man*, Oxford University Press, London, 1962, pp. xii+122, 12s. 6d.

Dr Thorpe's Riddell lectures enabled him 'to inquire what are the difficulties which confront the typical biologist when he attempts, by a personal synthesis of his own experiences, to reach some firm foundations of faith'. The inquiry is, of course, biologically most expert and the difficulties though frankly recognised are largely overcome.

Toms, E., *Being, Negation and Logic*, Basil Blackwell, Oxford, 1962, pp. x + 125. 12s. 6d.

Toulmin, S. (Ed.), *Quanta and Reality*, Hutchinson, London, 1962, pp. 96, 18s.

A Symposium—Deals with various aspects of quantum physics today. Dr Mary Hesse writes on Models and Matter. Professors Bohm and Pryce discuss the theoretical implication of two explanatory theories. Professor Hanson sums up.

Toulmin, S. & Goodfield, J., *The Architecture of Matter*, Hutchinson, London, 1962, pp. 399, 45s.

A history of 'matter-theory'—the physics, chemistry, and physiology of material things from the beginning of science to the present day.

Turbayne, C. M., *The Myth of Metaphor*, Yale University Press, 1962, pp. x+224, 48s.

Wells, M. J., *Brain and Behaviour in Cephalopods*, Heinemann, London, 1962, pp. xiii+171, 16s.

Williamson, A. A., *Biological Forces in World Affairs*, Public Affairs Press, Washington, 1961, pp. vi+58, $1.00

Whyte, L. L., *The Unconscious before Freud*, Tavistock Publications, London, 1962, xiii+219, 25s.

(*b*) ARTICLES

Biological Sciences

Barricelli, N. A., 'Numerical testing of evolution theories. Part I', *Acta Biotheoretica*, 1962, **16**, 57–69

Emerson, A. E., 'The Impact of Darwin on Biology', *Acta Biotheoretica*, 1962, **15**, 175–216

nts, F. A., 'Théories de l'heterosis et relations karyocytoplasmiques', *Acta Biotheoretica*, 1962, **16**, 1–27

urwkoop, P. D., 'The "organization centre"', *Acta Biotheoretica*, 1962, **16**, 27–57

wid, K., 'Problems of Methodology in the Boundary Area between Ecology and Genetics', *Stud. Filoz.*, 1962, **30**

Sciences

ynek, Z., 'Physical Determinism', *Stud. Filoz.*, 1962, **30**

i, S., Plebanski, J., 'Time, Space, Gravity and Microphysics', *Stud. Filoz.*, 1, **25**

. M., 'Atomism in Late Nineteenth-Century Physical Chemistry', *J. Hist. s*, 1963, **24**, 106–114

aum, A., 'The Special Theory of Relativity as a Case Study of the Importance of the Philosophy of Science for the History of Science', *Annali di Matematica Pura ed Applicata*, 1962, **57**, 257–282

Grünbaum, A., 'The Relevance of Philosophy to the History of the Special Theory of Relativity', *J. Phil.* 1962, **59**, 561–574

Ianochi, L., ' Une analyse philosophique de la théorie de la relativité restreinte ', *Vopr. Filos.*, 1961, **15**, 101–117

Poppei, G., ' Die Entwicklungsgesetzlichkeit der elementaren Synthese und Zerfallsprozesse ', *Dtsch. Z. Philos.*, 1961, **9**, 860–873

Rocca, M., ' De la mécanique de Schroedinger à la chimie de Pauling ', *Sophia*, 1963, **31**, 29–41

Simons, L., ' The Reduction of Temperature ', *J. Phil.*, 1962, **59**, 365–370

Vigier, J. P., ' Certaines questions méthodologiques de la théorie des particules élémentaires ', *Vopr. Filos.*, 1961, **15**, 119–122

Social Sciences

Lesche, C., ' A Metascientific Study of Psychosomatic Theories and their Application in Medicine ', *Interdisciplinary Stud. Scand. Summer Sch.*, 1962, **10**, 5–64

Nissen, I., ' Absolute Monogamy. The Attitude of Woman and War ', H. Aschehoug & Co. (W. Nygaard) Oslo, 1961, 10–72

Thompson, J. W., ' Meaningful and Unmeaningful Rotation of Factors ', *Psych. Bull.*, May 1962

Epistemology

Anderson, A. R., ' On Professor Martin's Beliefs ', *J. Phil.*, 1962, **59**, 600–606

Martin, R. M., ' On Knowing, Believing, Thinking ', *J. Phil.*, 1962, **59**, 586–600

General

Ardley, G. W. R., ' Berkeley's Philosophy of Nature ', *Univ. of Auckland Publ.*, 1962, **63**

Feyerabend, P. K., ' Metascience ', *Philos. Rev.*, 1961, **70**, 396–406

Greene, R. A., ' Henry More and Robert Boyle on the Spirit of Nature ', *J. Hist. Ideas*, 1962, **23**, 451–474

Grünbaum, A., ' Science and Man ', *Perspectives in Biology and Medicine*, 1962, **5** 483–502

Grünbaum, A., ' Whitehead's Philosophy of Science ', *Philos. Rev.*, **71**, 218–229

Hanson, N. R., ' The Irrelevance of History of Science to the Philosophy of Scienc *J. Phil.*, 1962, **59**, 574–585

Harré, R., ' Dimensions of Generality ', *Ratio*, 1962, **4**, 143–156

Kaminski, S., ' O niektorych unwarunkowaniach wspotczesnej filozofii na *Sesz. Nauk. Katol. Univ. Lubel*, 1961, **4**, 75–82

Lauriola, L., ' Dialettica della nature eleggi scientifiche ', *Studium*, 1961, **57**, 5

Mejbaum, W., ' Laws and their Expressions ', *Stud. Filoz.*, 1962, **31**

Stuermann, W. E., ' Science, Logic and Sanity ', *ETC*, 1962, **19**, 299–314

Logic & Methodology

Amsterdamski, S., ' Objective Interpretations of Empirical Probabilistic Sente *Stud. Filoz.*, 1962, **30**

Gordon, M., ' The " Neurophysiological " Justification of Induction ', *Stud. Filo* 1962, **30**

Lubnicki, N., ' On Final Justification ', *Stud. Filoz.*, 1962, **31**

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Sulowski, J., 'William of Conches and the Rudiments of Natural Method', *Stud. Filoz.*, 1962, 31

Szaniawski, K., 'A pragmatic Justification of rules of Statistical Inference', *Stud. Filoz.*, 1962, 31

Vincent, R. H., 'Popper on Qualitative Confirmation and Disconfirmation', *Aust. J. Phil.*, 1962, 2; 159–166

ABSTRACTS

Philosophy of Science, 1962, 29, No. 1

A. Landé, ' The Case against Quantum Duality '

(1) The idea that diffraction of matter particles can only be understood in terms of a temporary wave transformation or ' double manifestation ' is an uneconomical *ad hoc* hypothesis, shattered already in 1923 by the unitary quantum theory of diffraction of Duane which in 1926 became part of the quantum mechanics, with a statistical interpretation of wave-like appearances.

(2) Bohr's re-interpretation of Heisenberg's uncertainty of prediction as an indeterminacy of existence rests on an illegitimate literal translation of a wave result into particle language which is at variance with experience as well as with the statistical interpretation.

(3) The fact that one can transform the simple and unitary particle mechanics into a complicated wavelike form is only a weak substitute for genuine dualism.

(4) The strongest argument against a symmetry of the particle and the wave theory of matter is the explainability of the former in terms of simple postulates of invariance, leaving the wave formalism as a purely *ad hoc* construction.

W. Sellars, 'Naming and Saying '

The essay adopts the Tractarian view that configurations of objects are expressed by configurations of names. Two alternatives are considered: The objects in atomic facts are (1) without exception *particulars*; (2) one or more particulars plus a *universal* (Gustav Bergmann). On (1) a mode of configuration is always an empirical relation: on (2) it is the logical nexus of ' exemplification'. It is argued that (1) is both Wittgenstein's view in the *Tractatus* and correct.

N. R. Hanson, ' The Dematerialisation of Matter '

1. The philosophical version of the primary-secondary distinction concerns (*a*)
' real ' properties of matter, (b) the epistemology of sensation, and (*c*) a con
challenged by Berkely as illusory.

The scientific version of the primary-secondary distinction concerns (*a*
physical properties of matter, (*b*') a contrast essential within the history of at
and (*c*') a contrast challeged by 20th century microphysics as *de facto* untenabl

2. The primary-secondary distinction within physics can be interpreted
ways:

a. it can refer to content. Or *b*. it can refer to form.

2*a*. is simply false when seventeenth, eighteenth, or nineteenth century value
property-variables are introduced. 2*b*. is either uninformative or misleadin

I. Levi, ' On the Seriousness of Mistakes '

Several authors have recently contended that modern statistical theory provid
powerful argument in favour of the view that if scientists accept or reject hypotheses at all they do so only in a behavioural sense. In this paper, the argument from statistics

in favour of a behavioural view is outlined; an interpretation of two statistical procedures (Bayes method and significance testing) is offered which does not entail a behavioural analysis of ' accepting a hypothesis '; and the conclusion that non-behavioural analyses of belief are compatible with the application of current statistical theory in the sciences is tentatively advanced.

E. H. Madden and M. J. Kitely, ' Postulates and Meaning '

Various postulational views of meaning including those of Carnap are critically discussed. It is felt that there is something misguided in searching at all for such a criterion.

Philosophy of Science, 1962, **29**, No. 2

G. A. De Laguna, ' The Role of Teleonomy in Evolution '

The present thesis is that each of the three recognised phases of universal evolution: inorganic, organic, and post-organic initiated by the advent of man and his culture, is characterised and made possible by the emergence of a distinctive type of teleonomic organisation. While the process of evolutionary change is not itself end-directed to any goal, the end-directed structures which arise are not only ' in nature ' but are themselves crucial factors in evolution, since it is on them that natural selection operates. By using the concept of teleonomy, it is argued, one can avoid the issue of ' mechanism ' versus ' teleology '.

G. Maxwell, ' Theories, Frameworks, and Ontology '

Utilizing Carnap's notions of ' questions (or assertions) within a framework ' and ' questions (or assertions) about a framework ' and his account of A-truth (analyticity in the broad sense), a theory of the ontological status of entities—in particular, that of theoretical entities—is put forward.

In addition to the usual L-rules, each conceptual framework considered embodies
set of sentences whose truth value is quickly decidable on other than purely lingu-
ic grounds, a set of A-true formulae, and a set of rules for the confirmation and dis-
nfirmation of non-L-true, non-A-true formulae which are not quickly decidable.
teria which acceptable sets of A-true formulae must fulfil are given.
Using this apparatus, a schema for a realistic interpretation of scientific theories
tlined.

Walsh, ' Whewell on Necessity '
hewell's conception of necessary truth evolved only gradually; his early
nts are misleading. For this reason, and other reasons, he is commonly
t to have used the term ' necessary ' in the sense of ' absolutely necessary '.
ued that, on the contrary, the term is essentially relational in his mature view.
nclusion leads, in turn, to a re-interpretation of his doctrine of ' fundamental
'. The crux of his position is that cognitive principles which are initially regu-
ve become constitutive ones.

A. Grünbaum, ' Temporally-Asymmetric Principles, Parity between Explanation and Prediction, and Mechanism versus Teleology '

Three major ways in which temporal asymmetries enter into scientific induction are discussed:

1. An account is given of the physical basis for the temporal asymmetry of recorsability. Purported counter-examples to the asymmetry of spontaneous recordability are refuted.

2. It is shown how in cases of symmetric recordability, the associated retrodictive-prediction asymmetry makes for an asymmetry of assertibility as between an explanandum (or an explanans) refering to a future event and one referring to a past one. It is then contended that the failure to distinguish between an epistemological and a logical asymmetry vitiates the critiques of the Popper-Hempel thesis, which affirms symmetry of inferability as between predictive and post-explanatory arguments. In reply to Scriven, it is maintained that predictions based on mere indicators (rather than cause) do not establish an asymmetry in scientific understanding as between predictive arguments and post-explanatory ones.

3. A set of sufficient conditions are stated for the correctness of philosophical mechanism as opposed to teleology.

L. J. Goldstein, 'Evidence and Events in History'

A distinction is made between a real past which has nothing to do with historical events and an historical past made up of hypothetical events introduced for the purpose of explaining historical evidence. It is noted that the historical event is often brought in to explain the particular constellation of different kinds of historical evidence which are judged to belong together. The explanation of events must presuppose general laws. Subjective intentions of individuals are irrelevant to historical explanation.

K. L. Shewmaker and C. W. Berenda, 'Science and the Problem of psi'

Some issues raised by parapsychological phenomena (psi) are examined in the light of their implications for philosophy of science. Psi may be a product of the fact that all of our scientific concepts are abstractions and therefore oversimplifications Therefore possibly our best conceptual technique for dealing with psi is a no discursive symbolism, since this would not demand 'classes' (oversimplification).

Philosophy of Science, 1962, 29, No. 3

D. J. Hillman, 'The Measurement of Simplicity'

Various formulations of the principle of simplicity in science are examin rejected in favour of Goodman's proposal, the essence of which is to conc attention upon the predicates that form the extralogical basis of any given the to provide measures for comparing the relative structural simplicity of diffe of such predicates. The postulational basis of Goodman's method is set explained, together with some important amendments and additions, and a of theorems are proved, with whose aid the simplest theory to account for a corpus of scientific phenomena is readily determinable.

R. R. Delgado and J. M. R. Delgado, 'An Objective Approach to Measurement of Behaviour'

Theoretical problems concerning concepts of systems and measurement of behaviour were encountered during experimental studies of the effects of electrical stimulation of the brain on the social behaviour of a monkey colony. General problems involved in the description and measurement of behaviour of natural systems, and especially of organisms are discussed.

H. Törnebohm, 'The Lorentz-Formulae and the Metrical Principle'

The Lorentz-formulae are deduced from three factual statements the physical meaning of which is explained in terms of operations with clocks, light-signals and measuring rods. These statements are: (1) The time-length of a process is invariant. (2) The velocity of light is the same in all inertial systems. (3) The velocity of light is independent of the source. These statements can be deduced from the Lorentz-formulae. They are the physical content of the latter. The principle of relativity and the light-principle together, however, contain more phsyical meaning than the Lorentz-formulae. Operational definitions are given for all relevant concepts.

H. T. Walsh, 'Whewell and Mill on Induction'

Much of the Mill-Whewell dispute was purely verbal, but much was not. Mill did not understand Whewell; the true character of the non-verbal aspect of the controversy emerges only upon adequate analysis of Whewell's actual position. Ultimately, the dispute has to do with the given; neither man gives an adequate account of it. For this reason, the controversy cannot be resolved definitively in favour of either of them.

C. M. Myers, 'Perceptual Events, States, and Processes'

The notion that there is a category mistake or some other conceptual confusion in regarding seeing, hearing, and other forms of perception as events, states, or processes is incorrect. Ryle's analysis of 'seeing' as an achievement word does not rule out ur regarding seeing as an event, but in fact suggests that we do so when we carry he analysis beyond the point where Ryle leaves it. Perceptual events, states, and ocesses have both a physical aspect and an epistemological aspect. Any attempt educe one of these aspects to the other would involve a category mistake.

Philsophy of Science, 1962, **29**, No. 4

Hodges, 'The Dual Character of Marxian Social Science'

basic importance is the dual commitment of socialist humanism to both the n good and the partisan interests of a particular class, and the dual character ialist dialectics in its effort to be scientifically impartial and intellectually at the same time.

ick, 'Marxism and the New Physics'

e is no connection between Marxism and physical theory, whether deter-c or indeterministic. Marxism deals with physical theory only in so far as it is used for specific class purposes instead of social needs. Marxism does not derive its social theory, as has been asserted, either from, or by analogy with, physical processes, nor does it read 'social laws' of development into nature. The attempt to do so by way of ' dialectical materialism ' must be regarded as a Marxist aberration. The author deals with the history of this aberration and with the reasons for its persistency in Marxism-Leninism.

B. Dunham, ' On Teaching Marxist Epistemology '

The epistemology of Dialectical Materialism (Marxism) is the view that truth (i.e. the correspondence of a sentence with fact) can be determined by the following rule: 'Examine any alleged state of affairs as related to and distinguished from a total environment, and you will know whether or not the sentence alleging that state of affairs is true.'

No special pedagogy is required for Marxist epistemology.

H. J. Ehrlich, ' Some Observations on the Neglect of the Sociology of Science '

This paper represents an attempt to analyse the basis for the lack of interest and study in the sociology of science within American sociology and within American society.

L. Goodwin, 'The Historical-Philosophical Basis for Uniting Social Science with Social Problem-Solving '

Social scientific development has been greatly influenced by Galilean-Newtonian thought which emphasised formulation of abstract hypotheses valid throughout all time and space and independent of human characteristics. The uniting of certain aspects of Galilean-Newtonian and Hegelian-Marxian thought provides a genuinely experimental social science in which abstract hypothesis-testing is united with social action that is based on persons' awareness of relevant hypotheses viewed in historical perspective.

V. J. McGill, ' On Establishing Necessary Human Abilities and Disabilities '

This is a discussion of the claim by certain recent philosophers to have established universal human abilities and disabilities on 'logical' grounds, or as *a priori* necessary. The author argues, using a series of examples, that these supposed traits do, or coul
vary with empirical conditions, and that it is unlikely that in this psychological ar
there is any good alternative to inductive methods.

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The British Journal for the Philosophy of Science

VOLUME XIV August, 1963 No. 54

THEORETICAL TERMS AND PARTIAL INTERPRETATION *

PETER ACHINSTEIN

I

ACCORDING to a view widely held among logical empiricists, two sorts of terms may occur in a scientific theory. Those of the first kind, such as ' is blue ', ' is warm ', and ' is to the left of ', which refer to observable properties, are called *observational terms.* Those in the second category, such as ' contains one electron ', ' is a mutant gene ', and ' has a critical superego ', which designate unobservable properties, receive the title *theoretical terms.* Since terms of the latter sort do not refer to observables, a major concern of those philosophers who emphasise this distinction is to provide an explanation of what empirical meaning theoretical terms may enjoy. This has been an important issue in the programme to outline the foundations of an acceptable empiricism. Moreover, theoretical terms in scientific theories are presumably different in character and function from terms in speculative metaphysics, such as World Spirit ', ' manifests the Absolute ', and 'posits the Non-Ego ', hich also do not designate observable objects or properties. Thus, ical empiricists seek to provide a framework within which theoretical ns can be distinguished from metaphysical ones.

One major philosophical position on the nature of theoretical terms oposed by Hempel,[1] Carnap,[2] Braithwaite,[3] Nagel,[4] and others—I

Read, in abbreviated form, at the Western Division meeting of the American ophical Association in May 1962.

arl G. Hempel, ' The Theoretician's Dilemma ', *Minnesota Studies in the phy of Science*, ed. Herbert Feigl, Michael Scriven, and Grover Maxwell, polis, 1958, vol. 2, pp. 37–98.

dolf Carnap, ' The Methodological Character of Theoretical Concepts ', *esota Studies in the Philosophy of Science*, ed. Herbert Feigl and Michael Scriven, Minneapolis, 1956, vol. 1, pp. 38–76.

[3] R. B. Braithwaite, *Scientific Explanation*, Cambridge, England, 1955, Chapter 3.

[4] Ernest Nagel, *The Structure of Science*, New York, 1961, Chapter 5.

shall call the thesis of Partial Interpretation. Those who support this thesis claim to have provided an adequate analysis of the empirical nature of theoretical terms and the non-empirical character of metaphysical ones.[1] It is the purpose of this paper to examine these claims and present several considerations which cast doubt on their legitimacy.

2

Broadly speaking, the position of those who support the thesis of Partial Interpretation is this. Basic terms in the observational vocabulary of a scientific theory receive an empirical interpretation by means of semantical rules. Such rules, which are formulated in a suitable metalanguage, stipulate that a given term designates a certain observable property. Other terms in the observational vocabulary may then be introduced by utilising these interpreted terms. However, according to the thesis of Partial Interpretation, no semantical rules are given for terms in the theoretical vocabulary of a science; such terms receive no direct empirical interpretation. The empirical significance of theoretical terms results, rather, from their position in the scientific theory considered as a whole. Such terms gain an 'indirect' and 'partial' empirical meaning in virtue of the fact that by means of certain postulates of the theory they are related to sets of observational terms. For example, it is in virtue of a postulate which connects a sentence containing the theoretical term 'electron' to a sentence containing the observational term 'spectral line' that the former theoretical term gains empirical meaning within the Bohr theory of the atom. In an unem pirical theory of speculative metaphysics, on the other hand, postula do not connect metaphysical terms to observational ones, or at least such relationships are clearly outlined. For this reason, the metaphys terms lack empirical significance.

Those postulates of a scientific theory whose function it is to co certain theoretical terms, or sets of them, with observational ter called *rules of correspondence*. Proponents of Partial Interpre emphasise the necessity of admitting that in many scientific theor connections between theoretical and observational terms are weak and may take a wide variety of forms. A number of current pa the subject[2] refer to a position originally propounded in the 19 N. R. Campbell,[3] according to which statements containing theore terms may be related as wholes to statements containing only observational terms; such a relationship would suffice to confer empirical significance upon the theoretical terms involved. More recently, along the same lines, Hempel has suggested that statements, of whatever logical form, which contain essential occurrences of theoretical and observational terms, and hence indicate relationships between such terms, be counted as rules of correspondence.[1] This proposal would seem acceptable to other authors as well.[2]

---

[1] See Carnap, op. cit. pp. 54 ff.

[2] Hempel, op. cit. and Nagel, op. cit.

[3] N. R. Campbell, *Physics: The Elements*, Cambridge, England, 1920, Chapter 6

In what follows, then, a correspondence rule for a theoretical term M will be understood to be any statement containing at least one essential occurrence of M together with essential occurrences of observational terms and possibly other theoretical terms. We may now formulate the thesis of Partial Interpretation in the following manner, which is suggested in a number of passages by the authors in question:[3]

> (I) A theoretical term M, which occurs in a theory T, has empirical meaning if and only if there is within the theory a correspondence rule for M.[4]

Advocates of Partial Interpretation emphasise that the empirical meaning which a correspondence rule confers upon a theoretical term will in the usual case be 'partial' (i.e. no complete set of necessary and sufficient observational conditions will be given for the term). Nevertheless, according to the position in question, the important point is that a correspondence rule for a term does suffice to confer some measure of empirical meaning upon that term: this is what guarantees it at east a certain amount of cognitive significance and differentiates it om terms in a non-empirical theory of speculative metaphysics.[5]

---

[1] Hempel, op. cit. p. 72. With reference to a given theory T, any set of statements, cally compatible with T, which contains essential occurrences of all theoretical s employed by T, as well as essential occurrences of observational terms, is called empel an *interpretative system* for T (with respect to the observational terms in on), since these statements confer a partial empirical interpretation upon the tical terms involved.

ee Carnap, op. cit. pp. 46 ff.; Nagel, op. cit. pp. 100 ff.

arnap, op. cit. pp. 46–48; Hempel, op. cit. pp. 70, 72; Nagel, op. cit. pp. 90, 93; waite, op. cit. pp. 51 f.

rictly speaking, this thesis, as well as those to be considered subsequently, be understood as relativised to a language containing an observational vocaary and a theoretical vocabulary.

[5] The following discussion is intended to be neutral as between those who hold that the notion of empirical meaning is a matter of degree and those who claim that the meaningful-meaningless distinction is absolute. In either case, proponents of Partial

Presumably, the larger the set of observational terms to which a theoretical term is connected by means of correspondence rules, the more complete its empirical meaning.

This formulation of Partial Interpretation is, however, objectionable. For it implies that any uninterpreted term in any set of postulates whatever has empirical meaning, whether or not correspondence rules have been explicitly stated for the term. To see that this is so, imagine a theory whose postulates contain theoretical terms. From this theory choose any postulate T which contains a theoretical term M. T logically implies $O \supset T$, where O is any observational statement you please (i.e. any statement all of whose descriptive terms are observational). But the statement $O \supset T$ is a correspondence rule for M, since it contains both M and observational terms. Hence, according to thesis (I), the term M has empirical meaning. But every theoretical term in a theory occurs in at least one postulate of the theory, and the above argument can be repeated for each such postulate. Consequently, the theory contains a correspondence rule for each of its theoretical terms. It follows that no matter what theoretical terms are employed in a theory, and no matter what the postulates of the theory might be, every theoretical term necessarily has empirical meaning, on thesis (I). Furthermore, since a theory will contain many correspondence rules of the type in question for each theoretical postulate, the theoretical terms involved would seem to enjoy a considerable measure of empirical meaning.[1]

Such a latitudinarian criterion, which guarantees empirical meaning for every term in a theory, regardless of the nature of the theory, implies of course, that terms in speculative metaphysics also enjoy empiric

---

Interpretation seem to agree that correspondence rules supply theoretical terms w
some measure of empirical significance, and this is all that is required for the ens
examination.

[1] Exactly the same consequences will ensue even with the following som
stronger thesis of Partial Interpretation which utilises Hempel's notion of an
pretative system (see note 3 p. 91):

(I′) A theoretical term M, which occurs in a theory T, has empirical meanin
only if there is an interpretative system for T (which contains M esse

To see that this formulation fails to escape the difficulties besetting (I), consi
theory T containing postulates $T_1, \ldots, T_n$. Let $O_1, \ldots, O_n$ be any obser
statements which are logically independent of each other and T. The sent
$O_1 \supset T_1, \ldots, O_n \supset T_n$ are of course implied by, and hence contained in, the theory T. But this set of sentences constitutes an interpretative system for T, and thus, according to thesis (I′), guarantees empirical meaning for any theoretical term M in T.

meaning. For we have only to think of a metaphysical theory as a set of postulates containing essential occurrences of metaphysical terms which have been given no empirical semantical rules (i.e. semantical rules which stipulate that a term designates an observable property). In accordance with thesis (I), any term which occurs in any metaphysical postulate T has empirical meaning, since T implies the correspondence rule O $\supset$ T, where O is any observational statement you please. And, within the metaphysical theory, there will be a set of correspondence rules containing all the metaphysical terms as well as observational ones, thus assuring empirical meaning for the metaphysical terms.

Accordingly, the initial formulation of the thesis of Partial Interpretation appears inadequate.

3

At this point two possibilities seem open to the proponent of Partial Interpretation. He might modify the notion of a correspondence rule, in an attempt to preclude the type of statements used in the previous examples from the realm of correspondence rules. Or, he might propose additional requirements which a theoretical term must satisfy if it is to have empirical meaning.

Although one might be tempted to suggest that Hempel's notion of a correspondence rule be modified, any such modification which would preclude correspondence rules of the type illustrated above would be rejected as undesirable by most advocates of Partial Interpretation. For a statement of the form O $\supset$ T, where O is an observational statement and T a theoretical one, provides an observational sufficient condition for the truth of a theoretical statement. The occurrence of correspondence rules in the sciences which have this form is maintained by various authors.[1] Moreover, even if we require a stronger type of correspondence rule, e.g. one which involves quantifiers and which relates terms rather than entire sentences, the same difficulties emerge. Thus, consider the postulate $(x)(M_1x \supset M_2x)$, which contains only the theoretical terms M_1 and M_2. This postulate implies $(x)\ [Ax \supset (M_1x \supset M_2x)]$, where A is any observational term you please. But this statement is a correspondence rule even by the stricter criterion mentioned above, and thus it confers empirical meaning upon the theoretical terms M_1 and M_2. In a similar manner correspondence rules can be generated for all theoretical terms which occur in the postulates of a theory.

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[1] See Carnap, op. cit. p. 48; Hempel, op. cit. p. 72; Nagel, op. cit. p. 101.

For these reasons, it would seem that the most advisable course for the advocate of Partial Interpretation who wishes to retain the notion of a correspondence rule outlined by Hempel is to formulate additional specifications which a theoretical term must satisfy to have empirical meaning.

The previous difficulties arise, it might be argued, because the correspondence rules cited are logically implied by the postulates of the theory, even if these postulates contain nothing but theoretical terms. Referring to a postulate all of whose descriptive terms are theoretical as a *purely theoretical postulate*, the thesis of Partial Interpretation might be revised in the following manner:

> (II) A theoretical term M, which occurs in a theory T, has empirical meaning if and only if there is a correspondence rule for M which does not follow logically from the set of purely theoretical postulates of T.[1]

4

Although advocates of Partial Interpretation might wish to maintain that thesis (II) circumvents the previous type of difficulty, nevertheless a serious problem remains which besets this as well as the initial formulation.

By what amounts to a trivial addition to a set of postulates none of whose descriptive terms is observational, any term in this set can be given an empirical meaning in terms which include virtually every basic observational predicate in the language; yet it would seem unreasonable to claim that the non-observational terms in question gain empirical meaning from such a procedure. Let us see why this is so.

Consider any set of observational statements $O_1, O_2, \ldots, O_n$, all of which are known to be true. Such a set might include established empirical generalisations such as 'ravens are black', 'the sea is salty', and in addition simple existential truths such as 'something is green', 'something is warm', etc. (By including statements of the latter sort, virtually every basic observational term in the language can be employed in the set $O_1, O_2, \ldots, O_n$.) Consider now a speculative metaphysician who, although not especially concerned with ordinary empirical statements of the sort illustrated above, will doubtless be willing to admit their truth. Let us choose as our example of a true observational statement 'the sea is salty', to be designated O_1. Suppose that one of the postulates of the metaphysician's theory is the following, to be designated T_1:

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[1] Although it might seem plausible to stipulate in addition that each theoretical term must have an independent correspondence rule, i.e. one which does not follow deductively from other (non-logically equivalent) correspondence rules of the theory, in fact this would be too stringent a requirement. For it would imply that no correspondence rule (with the exception of the entire theory considered as a correspondence rule) could be said to confer any empirical meaning upon a theoretical term. To see that this is so, suppose that there is a correspondence rule C for a theoretical term M. Let T be a postulate of the theory which does not imply C. By definition, the conjunction T . C is also a correspondence rule for M, from which the correspondence rule C follows deductively. Hence, if the above requirement were added, C could not be said to confer empirical meaning upon the term M.

> The Absolute is each appearance, and is all, but it is not any one as such.[1]

If the metaphysician grants the truth of each of the sentences O_1 and T_1, then surely he is willing to admit the truth of the sentence $T_1 . O_1$, which expresses their conjunction. But since $T_1 . O_1$ satisfies the conditions for being a correspondence rule for the metaphysical term 'the Absolute', by adding this sentence to his theory the metaphysician will, in accordance with the previous formulations of the thesis of Partial Interpretation, provide empirical meaning for one of his fundamental terms. A similar procedure is possible with respect to every metaphysical term in his theory. Thus, if $T_1, T_2, \ldots, T_n$ are purely metaphysical postulates, and $O_1, O_2, \ldots, O_n$ are any observational truths whatever, the metaphysician has but to embellish his theory with the correspondence rules $T_1 . O_1, T_2 . O_2, \ldots, T_n . O_n$ in order to meet the requirements of providing empirical meaning for all the metaphysical terms in his postulates. Moreover, when these correspondence rules are conjoined, each metaphysical term is related to a very large number of observational terms, and thus would seem to gain in empirical meaning.

The procedure of adding such correspondence rules to his theory would surely not be objectionable to the speculative metaphysician whose interest after all lies in developing the purely metaphysical part of his theory and not in generating observational consequences, or at least not observational consequences of the sort made possible by the type of correspondence rules in question. Furthermore, since all observational consequences afforded by the correspondence rules are true, the world of experience will have no more effect upon the truth of the purely metaphysical postulates than it previously did.[2]

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[1] F. H. Bradley, *Appearance and Reality*, Oxford, 1897, p. 431

[2] It is not being claimed here that according to preceding formulations of Partial Interpretation, truth of the observational statements is required in order that the

Finally, we must ask whether it is reasonable to maintain that such a procedure, if adopted by a speculative metaphysician, would guarantee empirical meaning for the individual metaphysical terms of his theory. One who maintains the thesis of Partial Interpretation in either of its previous forms is proposing what Carnap calls an *explication* of the concept of empirical meaning of theoretical terms, i.e. an analysis that provides a precisely determined meaning for a concept which in ordinary use (or in ordinary scientific use) is somewhat vague.[1] In accordance with the procedure outlined by Carnap, the adequacy of a proposed explication is determined in part by considering the more important features of the ' preanalytic ' concept (i.e. the concept as it is understood prior to explication). In the case of the preanalytic concept of empirical meaning, we would want to deny empirical meaning to the term ' the Absolute ', as it occurs in a theory of speculative metaphysics, if such a theory provides no observational clues for recognising the Absolute, or testing any of its properties. Whereas, we would grant empirical meaning, for example, to the theoretical term ' conducts electricity ', as this term occurs in physical theory, since there are observational procedures for recognising an electrical conductor, determining the presence of an electric current, and testing the properties of electricity. Admittedly, the notions of empirical meaning to which we here refer need more precise formulations. However, an explication of the concept of empirical meaning will be judged in part by its success in capturing and making exact such preanalytic notions as these.

According to the proponent of Partial Interpretation, by connecting a sentence containing non-observational terms to a sentence containing observational terms, the non-observational terms in the former sentence indirectly acquire empirical meaning. Thus, in accordance with previous formulations of this thesis, it is possible for sentences such as the following to confer empirical meaning upon the metaphysical terms which they contain:

> The Absolute is each appearance, and is all, but it is not any one as such, and the sea is salty.

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non-observational terms involved have empirical meaning: to satisfy the previous criteria of meaningfulness the metaphysician could be allowed to add any observational statements to his theory regardless of their truth-value. The point of adding only truths is merely to ensure that the metaphysician cannot reject the above procedure on the ground that his theory is made to yield false consequences.

[1] Carnap, *Logical Foundations of Probability*, Chicago, 1950, Chapter 1

The Absolute, etc., and the sky is blue.
The Absolute, etc., and grass is green.[1]

However, sentences of this sort obviously fail to provide any clue whatever of how to recognise the Absolute, test for its presence in a given situation, or determine any of its properties. Nor will the metaphysician's theory of the Absolute, when combined with these sentences, furnish the desired information, if his theory contains only non-observational terms.

These considerations strongly suggest the principle that if a sentence which contains no observational terms is connected by means of conjunction to another sentence all of whose terms are observational, the terms in the former sentence gain no empirical meaning from such a procedure. This principle, which would seem to be one that a criterion of empirical meaning should be required to satisfy, can be stated more precisely as follows :

> *Principle* 1:
>
> If a sentence S (in a theory T) is a conjunction of two sentences S_1 and S_2, where none of the descriptive terms in S_1 is observational and each of the descriptive terms in S_2 is observational, then the occurrence of S (in T) does not suffice to guarantee that the non-observational terms in S_1 (and therefore in S) have empirical meaning.

In accordance with this principle, the non-observational term 'the Absolute' cannot be shown to have empirical meaning by citing merely a conjunction one of whose member-sentences contains only non-observational terms (including the term 'the Absolute') while the other member-sentence contains only observational terms.

It should be noted, moreover, that a number of advocates of Partial Interpretation have themselves appeared to maintain this principle. According to a condition of adequacy for a definition of empirical significance outlined by Hempel, 'If under a given criterion of cognitive significance, a sentence N is non-significant, then so must be all truth-functional compound sentences in which N occurs non-vacuously as a component.'[2] If N contains only non-observational terms which, by hypothesis, have no semantical rules, presumably Hempel would agree that the sentence N considered by itself is non-significant; and since such a sentence N, according to Hempel's condition of adequacy, gains no significance when conjoined with an observational sentence, the terms in N should also gain no significance.[1] Another writer committed to Principle 1 is Carnap, who implies in one passage that if a sentence containing any empirically meaningful term is connected in a separable way, e.g. by conjunction, to a sentence with another term, it is not the case that empirical meaning is conferred upon the second term.[2]

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[1] We might imagine these sentences to be correspondence rules for a theory which contains postulates involving the term 'the Absolute' as well as other non-observational terms.

[2] Hempel, 'The Concept of Cognitive Significance: A Reconsideration', *Proceedings of the American Academy of Arts and Sciences*, 1951, 80, 62

It is readily seen, however, that the previous formulations of the thesis of Partial Interpretation violate Principle 1. For if a theory includes among its postulates a sentence T, which contains only non-observational terms, and a sentence O, which contains only observational terms, then by the previous formulations of Partial Interpretation, the terms in T can be shown to have empirical meaning, since the sentence T . O is a correspondence rule for the theory and hence guarantees empirical meaning for the terms in T. However, according to Principle 1, the terms in T cannot be shown to have empirical meaning by citing their occurrence in the conjunction T . O.

For these reasons the preceding formulations of the thesis of Partial Interpretation seem objectionable. First, by supplementing a metaphysical theory with a set of innocuous propositions which provide no observational information about the metaphysical terms, the metaphysician can nevertheless claim empirical meaning for all his terms. Secondly, the previous formulations of Partial Interpretation are incompatible with a principle of empirical meaning which sound, and seems to be accepted by some of the strongest advocates Partial Interpretation.

5

At this point it might be suggested that the concept of empirical meaning has been treated too narrowly. Although testability of the item designated by a theoretical term is a sufficient condition for empirical meaningfulness of the term, it is not a necessary one. The essential matter, it might be argued, is not that terms in a theory should designate objects or properties whose presence can be detected, but rather that a term be such that from a postulate of the theory in which it occurs, together with other postulates of the theory, observational consequences can be deduced. In other words, it is their ' cash value ', measured by means of the observational conclusions they afford, which assures empirical meaning for theoretical terms in a theory. In this pragmatic spirit, the thesis of Partial Interpretation might be reformulated as follows:

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[1] It might be noted how much stronger is Hempel's criterion than Principle 1. For Hempel's condition of adequacy would seem to rule out as non-significant any truth-functional compound containing an observational sentence and a purely theoretical one.

[2] Carnap, 'The Methodological Character of Theoretical Concepts', p. 58

> (III) A theoretical term M, which occurs in a theory T, has empirical meaning if and only if there is in the theory a postulate P in which M occurs essentially, and there is an observational sentence which can be deduced from P together with other postulates and correspondence rules of the theory and cannot be deduced from these other postulates and correspondence rules alone.

Those who would wish to propose this formulation might argue that it renders the thesis of Partial Interpretation perfectly compatible with Principle 1. For, if a postulate T, which contains only non-observational terms, is connected by means of conjunction to a postulate O, which contains only observational terms, then, even though O will be an observational consequence of this enlarged theory, postulate T will be irrelevant for its derivation. Hence, according to thesis (III), it will not be possible to show that the terms in T have empirical significance by citing the conjunction T . O.

Unfortunately, however, this does not suffice to make thesis (III) acceptable. For although it does not violate Principle 1 taken by itself, thesis (III) is nevertheless incompatible with the conjunction of Principle 1 with the following principle, which would seem to be an unexceptionable one:

> *Principle 2*:
>
> If a sentence (or conjunction of sentences) S is such that the occurrence of a term M in S suffices to guarantee that M has empirical meaning, then the occurrence of M in any sentence logically equivalent to S also suffices to guarantee that M has empirical meaning. And if S is such that the occurrence of M in S does not suffice to guarantee that M has empirical meaning, then the occurrence of M in any sentence logically equivalent to S also does not suffice to guarantee that M has empirical meaning.

In other words, according to this principle, if a term can be shown to have empirical meaning by appealing to its occurrence in one linguistic context (one set of sentences), it must be possible to demonstrate that it has empirical meaning by appealing to its occurrence in any linguistic context exactly similar to the first (any set of sentences logically equivalent to the first set).

To show that thesis (III) is incompatible with Principles 1 and 2, let us turn to an example. Consider once more the sentences 'the Absolute is each appearance, and is all, but it is not any one as such', and 'the sea is salty,' to be designated T_1 and O_1, respectively. The conjunction of these sentences, $T_1 . O_1$, is logically equivalent to the sentence $T_1 . T_1 \supset O_1$. But the latter constitutes in effect a theory whose purely theoretical postulate is T_1, and whose correspondence rule is $T_1 \supset O_1$. This theory has O_1 as an observational consequence, and stated in such a way that T_1 and $T_1 \supset O_1$ are postulates of the theory, the postulate T_1 is required in order that O_1 be generated as a consequence. Hence, in accordance with thesis (III), the theory $T_1 . T_1 \supset O_1$ guarantees empirical meaning for the terms in T_1. But since the theory $T_1 . T_1 \supset O_1$ is logically equivalent to $T_1 . O_1$, which according to Principle 1 fails to guarantee empirical meaning for the terms in T_1, it follows from Principle 2 that the theory $T_1 . T_1 \supset O_1$ also does not suffice to guarantee empirical meaning for the terms in T_1. Thus thesis (III) is violated.

Furthermore, irrespective of Principle 1, it is readily seen that thesis (III) violates Principle 2 by itself. For according to thesis (III), in a theory whose postulates are just T and O, where T contains no observational terms and O contains only observational terms, the terms in T have no empirical meaning; whereas in the theory whose postulates are T and $T \supset O$, the terms in T do have empirical meaning. Since these theories are logically equivalent, Principle 2 is obviously violated.

For these reasons, if we wish to incorporate the notion of observational consequence into the thesis of Partial Interpretation, it wou[illegible] seem necessary to require a more felicitous formulation of this thesis. With this in mind let us turn our attention to a proposal of Carnap's.

Although in a number of passages Carnap suggests that the existence of a correspondence rule for a theoretical term suffices to confer empirical meaning upon it (i.e. thesis (I)), nevertheless he does wish to propose a more rigorous criterion than this. According to Carnap, a theoretical term M which occurs in a theory T has empirical meaning if, roughly speaking, an assumption about M which is added to the theory makes a difference for the prediction of an observable event. More specifically, Carnap's formulation of the thesis of Partial Interpretation may be expressed as follows:[1]

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[1] Carnap, 'The Methodological Character of Theoretical Concepts', p. 51

(IV) A theoretical term M, which occurs in a theory T, has empirical meaning if and only if the following conditions are satisfied: There is a sentence S_M which has M as its only descriptive predicate. There is a sentence S_K all of whose descriptive terms have empirical meaning.

The conjunction $S_M . S_K . T . C$ is logically consistent, where C is the set of correspondence rules of the theory.

There is a sentence S_O, containing only observational terms, which is logically implied by the conjunction $S_M . S_K . T . C$, but not by the conjunction $S_K . T . C$.

This formulation, however, is not immune to difficulties which beset thesis (III). To see that this is so, consider any theory T containing only non-observational terms. Let S_M be any sentence which is compatible with, but not deducible from, T, and whose only descriptive predicate is the theoretical term M. Let us add to theory T the correspondence rule $S_M \supset S_O$, where S_O is any factual statement all of whose descriptive terms are observational. It follows from Carnap's criterion that M has empirical meaning, since the conjunction of S_M with T and the correspondence rule $S_M \supset S_O$ implies the observational statement S_O, and S_O is not implied by the conjunction of T and the correspondence rule alone. That is, in accordance with thesis (IV), the occurrence of M in the sentence $S_M . T . S_M \supset S_O$ guarantees empirical meaning for M. Note, however, that $S_M . T . S_M \supset S_O$ is logically equivalent to the conjunction $S_M . T . S_O$. But in accordance with Principle 1, since all terms in S_M and T are by hypothesis non-observational, such terms cannot be shown to have empirical meaning by citing their occurrence in the conjunction $S_M . T . S_O$, and in particular the non-observational term M cannot be shown to have empirical meaning by invoking this conjunction. Since the conjunction $S_M . T . S_O$ is logically equivalent to $S_M . T . S_M \supset S_O$, it follows from Principle 2 that the occurrence of M in the latter conjunction also does not suffice to guarantee empirical meaning for M—thus violating Carnap's thesis (IV).

Moreover, thesis (IV) is incompatible with Principle 2 taken by itself. For, consider a theory containing the postulates T and $S_M \supset S_O$, where T contains only theoretical terms, S_M contains the theoretical term M and is compatible with, but does not follow from, T, and S_O is any observational statement whatever. According to thesis (IV), by adding the assumption S_M to this theory we show that the term M has empirical meaning, since from this assumption, together with the theory, the observational statement S_O follows, while it does not follow from the theory by itself. In other words, the occurrence of M in the conjunction $S_M . T . S_M \supset S_O$ suffices to guarantee that M has empirical meaning. Consider now the theory whose postulates are merely T and S_O. According to thesis (IV), by adding the assumption S_M to this theory (thus yielding $S_M . T . S_O$) the term M cannot thereby be shown to have empirical meaning, since the sentence S_O, the only observational consequence of this enlarged theory, does not depend for its derivation upon the assumption S_M. Thus, in accordance with thesis (IV), the occurrence of M in the conjunction $S_M . T . S_O$ does not suffice to guarantee empirical meaning for M. But the conjunctions $S_M . T . S_M \supset S_O$ and $S_M . T . S_O$ are logically equivalent. Therefore, according to Principle 2, if the term M can be shown to have empirical meaning by citing its occurrence in the first conjunction, it must also be possible to demonstrate that it has empirical meaning by citing its occurrence in the second conjunction—thus violating Carnap's thesis (IV).

Accordingly, if Principles 1 and 2 are acceptable, the thesis of Partial Interpretation, even under those formulations which take into account the observational consequences of a theory, stands in need of serious revision. This is not to deny, of course, that the possibility of deducing observational consequences from a theory in certain cases has an important bearing upon the empirical significance of terms in the theory. What is objectionable is the unrestricted use which the proponent of Partial Interpretation makes of the notion of observational consequence in theses (III) and (IV).

6

There is one final point I wish to raise. On all previous formulations of the thesis of Partial Interpretation, the task of constructing scientific theories will often turn out to be a matter of extreme triviality.

Suppose we seek to provide a theory which will explain a set of well-confirmed laws L, e.g. laws about the behaviour of gases. These laws are to be construed as general hypotheses containing only observational terms. To furnish the desired theory in a completely trivial manner, it suffices to construct any set of sentences T using only theoretical terms, and add $T \supset L$ as a correspondence rule. According to previous formulations of the thesis of Partial Interpretation, the theoretical terms in T have empirical meaning within this theory.[1] And since the empirical laws in question follow deductively from the theory, the laws can be said to be explained by the theory, on the account of explanation usually accepted by proponents of Partial Interpretation.[2] Surely this reduces scientific theory-construction to a task of painful triviality, making it one that can obviously be accomplished by anyone who knows little more than how to manipulate logical symbols.

In his paper 'The Theoretician's Dilemma', Hempel recognises this as a possible objection to Partial Interpretation, and suggests a reply.[3] First of all, says Hempel, a theory constructed in the trivial manner indicated would not be a 'potentially interesting theory', and hence presumably would not be a serious contender for an explanatory role. This remark by itself is puzzling, however, for according to an analysis of explanation proposed by Hempel himself, either a theory explains a given law or it does not: in this analysis no reference is made to any notion of how interesting the theory may be.

Hempel does go on to say, however, that the possibility of constructing theories along the lines indicated reminds us that in addition to having empirical significance, a scientific theory must satisfy other requirements as well: its observational consequences must be empirically well-confirmed; it must meet some criterion of logical simplicity; and it must suggest further empirical laws.

The additional criteria which Hempel suggests, however, pose no bstacle in the way of trivial theory-construction. First of all, the equirement that observational consequences be well-confirmed is tomatically satisfied, since the only observational consequences of the pe of theory constructed are the empirical generalisations to be plained, and these, by hypothesis, are well-confirmed. Secondly, if consider some standard criteria of simplicity, such as paucity of mptions, economy of postulated entities, or lack of complexity in

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arnap's thesis (IV) is an exception. But the identical effect can be achieved in e of thesis (IV), as follows. Construct the same theory T . T ⊃ L as above. ict a sentence S_M which contains M as its only theoretical term, and which is ible with, but does not follow from, T. Add to the theory the postulate , where S_O is any true observational sentence you please which does not follow from the theory. Do this for each theoretical term which occurs in T, where S_O may be different in each case. By the addition of such postulates, we are guaranteed that each theoretical term in the theory has empirical meaning.

[2] The so-called covering-law or deductive model of explanation. See Hempel and Oppenheim, 'Studies in the Logic of Explanation', *Philosophy of Science*, 1948, 15, 135-175

[3] p. 84

mathematical equations, the task of theory-construction remains trivial. To meet such requirements of simplicity, it is necessary merely to construct sentences in T which are few in number, or which utilise few theoretical terms, or whose mathematical expressions exhibit simplicity of form. Thirdly, if additional empirical laws must be 'suggested' by the theory, in the sense of 'deduced from' it, we have only to append to the consequent of the correspondence rule T $\supset$ L any observational laws desired. It might be noted that, in this manner, by adding empirical laws from various sciences the theory we construct becomes quite powerful in scope, since one set of theoretical postulates will then suffice to explain empirical laws in a wide variety of areas.

Accordingly, requirements such as confirmation of consequences, logical simplicity, and derivation of further laws, in no way preclude the type of trivial theory-construction which the thesis of Partial Interpretation makes possible.

Finally, in this connection, the proponent of Partial Interpretation might wish to suggest a proposal of N. R. Campbell's, which seems designed to meet the present difficulty. According to Campbell, although the methods outlined above will yield a theory, it will be a worthless one, since it will lack one essential feature, viz. an analogy or 'similarity in form' to known laws. Campbell writes: 'It is never difficult to find a theory which will explain the laws logically. What is difficult is to find one which will explain them logically and at the same time display the requisite analogy.'[1]

Campbell's remarks in the context of the present problem are of doubtful efficacy. For in constructing our purely theoretical postulat T, we may make them similar in form to any known laws we wis By taking as a correspondence rule the sentence T $\supset$ L, where L is t set of empirical generalisations to be explained, theory-constructi remains essentially as trivial a procedure as before.

7

Examination of the thesis of Partial Interpretation has reveale one usual formulation of this thesis, according to which a corr dence rule suffices to confer empirical meaning upon a theoretical term, leads to the unacceptable consequence that any non-observational term in any theory whatever enjoys a measure of empirical meaning. A more felicitous formulation, which purports to alleviate this difficulty, is nevertheless objectionable on the ground that with inessential modifications within a purely metaphysical theory, the terms in such a theory can be shown to have empirical meaning according to the requirements of Partial Interpretation, although such terms fail to satisfy basic preanalytic notions of empirical significance. Nor does the shift of emphasis to a thesis which takes into account observational consequences, as this thesis is expressed in the formulations considered, seem to be successful, since these formulations violate certain fundamental principles of empirical meaning. Finally, the thesis under examination appears to render the task of constructing scientific theories in many cases a matter of extreme triviality, making it capable of performance by any person, or machine, with a modicum of logic.

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[1] Campbell, op. cit., p. 130

Those who support the thesis of Partial Interpretation claim to provide an acceptable account of the empirical nature of theoretical terms within a science, and to distinguish such terms from those which might appear in a non-empirical theory of speculative metaphysics. If the considerations outlined in this paper are correct, these claims remain to be justified.

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THE STATUS OF DETERMINISM *

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AMONG the principles which Kant describes as 'regulative' is the determinist principle that every empirical state of affairs follows in accordance with causal laws from some earlier state of affairs. In calling this 'regulative', Kant is making a five-fold claim.

(1) The principle cannot be conclusively proved. Because the future is or may be endless, it is always possible that new states of affairs are going to come about in ways which do not accord with the principle.

(2) The principle cannot be conclusively disproved. Because logical space is limitless, it is always possible, however persistent our failure to find a causal explanation for a state of affairs, that there is an explanation which we have not yet found.

(3) Unlike some statements which can be neither settled nor ruled out by experience, the determinist principle is at least relevant to experience. Although we cannot reach the goal of showing it to be true, or that of showing it to be false, we can and do take small steps towards each goal. We move towards verifying the principle whenever we explain a state of affairs by means of a well-tested and unrefuted hypothesis; and we move towards falsifying it whenever w try but fail to find such an explanation. This sense of direction mar off the determinist principle sharply from such unverifiable and u falsifiable statements as that all change results from the exercise of divine will, or that there are intangible, invisible, and impo goblins.

(4) Sense can therefore be given to the notion of assenting t determinist principle. For it can be construed as a piece of a urging us always to move as far as possible in the direction of ve the principle, and never to despair of moving further. Assent principle is just acceptance of this advice.

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* A version of this paper was read to the conference of the British Societ Philosophy of Science which was held at Leeds in September 1962. Co made on that occasion have helped me to improve the paper in several I am particularly grateful for remarks by Professor R. B. Braithwaite and Lakatos.

(5) The principle gives good advice to anyone who wishes to understand the empirical world, for it advises him to pursue such understanding optimistically and therefore energetically; furthermore, precisely because the determinist principle cannot be falsified, it constitutes good advice *with no risks attached.* Advising someone to assume that there is an explanation for every state of affairs is not like advising a prospector to assume that there is gold in every river-bed. Acceptance of that advice might engender optimism and therefore industry, but it would also certainly lead to error, for some river-beds are known to contain no gold.

I want now to hold in one hand Kant's treatment of the determinist principle as ' regulative ', while holding in the other his claim that he has *proved* the so-called ' second analogy of experience ' which says: ' All alterations take place in conformity with the law of the connection of cause and effect ' (B 232). He makes it abundantly clear that he does not take this to mean merely that it is *safe and sensible* to say that all alterations take place . . . etc. The claim of the second analogy of experience is that to say there are uncaused events is to say something demonstrably false; and this is in conflict with the description of the determinist principle as ' regulative ', for that description implies that to say there are uncaused events is to say something which is objectionable because it may induce despondency in the scientific enquirer, *but which cannot be proved to be false.*

In fact, Kant does not succeed in proving the second analogy of experience; but he does (in my view) succeed in proving the more limited conclusion that there could not be intellectually graspable experience which was not experience of a world which manifested a fairly high degree of causal order. (I cannot present his proof of this here; it includes, among much else, all that is valid in Wittgenstein's argument against the possibility of a private language.) This limited conclusion implies that there is no possibility of our finding that the world has become altogether resistant to science; but it leaves open the possibility that some causally inexplicable states of affairs should come about, and so it is consistent with claiming a purely regulative virtue for the statement that there is a causal explanation to be found for every state of affairs.

There is, then, a double problem about Kant's handling of the second analogy. Why does he fail to see the gap between the weaker version which he does prove and the stronger version which he claims to have proved? And why does he fail to see that the claim to have proved the stronger version is inconsistent with the attribution, a few hundred pages further on in the *Critique*, of a regulative status to the determinist principle? This is a problem about Kant rather than about philosophy, but I hope to show that it shares with many other problems about Kant the merit that in discussing it we can learn something in philosophy. The real objection to the protective Kantian commentators whose main concern is to defend Kant from charges of error and inconsistency is that they take a real, human, confused, fertile philosopher from whom much can be learned and present him as a mythical, superhuman, infallible, sterile bore. For G. Bird (*Kant's Theory of Knowledge*, p. 71) has rebutted the accusa that Kant tells two different and mututally inconsistent stories about the status of the determinist principle, on the grounds that 'Kant takes the trouble to distinguish between' the determinist principle which he calls 'regulative' and that of the second analogy for which he claims a more than regulative status. This might be plausible if it were accompanied by an account of what these two principles are, and of how two principles, of which one is regulative and the other not, can both express some sort of determinism. But Bird tells us nothing about this. Furthermore, his only justification for saying that Kant 'takes the trouble to distinguish between' the determinism which is called regulative and the determinism of the second analogy which is not called regulative, is that Kant says things about the one which are inconsistent with things he says about the other. But this can be taken as the making of a distinction only if one takes it as literally *axiomatic* that Kant never contradicts himself. I find this axiom unacceptable, but my main objection to this smoothing-over approach is that it teaches us nothing about what is true in philosophy.

I hope it will not prove too exasperating if I set about solving (and learning from) this problem about what Kant is up to by raising first the different but related problem of what he is up to when he says that there is a radical error in Hume's analysis of the concept of cause. Of a group of concepts which includes that of cause, Kant says (A 112):

> All attempts to derive these . . . concepts . . . from experience, and so to ascribe to them a merely empirical origin, are entirely vain and useless. I need not insist upon the fact that, for instance, the concept of a cause involves the character of necessity, which no experience can yield. Experience does indeed show that one appearance customarily follows upon another, but not that this sequence is necessary . . .

Kant denies that the concept of cause can be derived from experience because derivation requires thought, and one can think about one's experience only if one already has a use for the concept of cause. But his concern here is less with the Humean account of how we *acquire* the concept of cause than with the Humean *analysis* of this concept. Because ' the concept of cause involves the character of necessity, which no experience can yield', no analysis of the concept in purely experiential terms can be correct.

Some writers have tried to fault Hume's analysis by producing statements which, they say, Hume is committed to calling causal laws but which we know perfectly well not to be causal laws: every time the 5 o'clock whistle blows at Dagenham, the workers at Coventry down their tools and go home. Such counter-examples may tell against specific empiricist analyses of causality, but they cannot show the wrongness of the empiricist *programme* for such an analysis. If we know perfectly well that this whistle's blowing does not cause those workers to down tools, the empiricist has only to ask *how* we know this, and to amend his analysis in the light of the answer. Kant makes many mistakes, but he does not make the mistake of trying to show, by the production of empirical counter-examples to specific empiricist analyses, that no empiricist analysis can succeed. His position is accurately stated by Körner (*Kant*, p. 74): ' While Kant insists that the notion of causality is not *equivalent* to that of regular succession, he holds that unless it *implied* this notion it could not refer to anything in perception '. But, although Kant thus seems to claim the true notion f causal necessity to be narrower than Hume says it is, he rightly frains from any attempt to prove or illustrate the over-generosity of me's concept by pointing to universal statements which Hume uld have to call causal but which are obviously not so.

ust because Kant does not attack Hume in this misguided way, there problem over what attack he does think he is launching. The on to this problem is twofold. One part of it is well known to n commentators; in respect of the other part—which alone is t to regulativeness—I claim a certain mild originality.

rst way in which Kant thinks that Hume cannot do justice to of causes brings us back to the part of the second analogy d in proving. Hume presents us as discovering orld continues to be fairly orderly, and total chaos is a possibility which nce; Kant, on the other hand, knows that causal order is not dispensable to this extent. It is often said that the burden of Kant's complaint against Hume is just this: Kant is accusing Hume of implicitly denying that *causal laws are necessary* in the sense that *there must be (known) causal laws* if there is to be graspable experience at all. But this is different from saying that *causal laws are necessary* in the sense that *every causal law expresses or involves some non-empirical element of necessity.* What looks like a complaint against Hume's analysis of the concept of cause is here presented as a complaint which is compatible with any analysis, namely that Hume has exaggerated the intellectual dispensability of causal order. Kant does sometimes seem to conflate these two complaints, but that is no excuse for his commentators' doing so. To take an example from one of the best of them, Walsh (*Reason and Experience*, p. 153) says: 'Kant is emphatically not saying that some kind of inner necessity binds cause to effect. . . . The necessity which marks the causal relation is . . . derived wholly from the necessity of the principle of causality itself.' This says that Kant brings only one charge against Hume, namely that concerning the dispensability of causal order; yet Walsh expresses this in terms of the phrase 'the necessity which marks the causal relation', which is appropriate only to the other anti-Humean charge which Walsh explicitly (and rightly) denies Kant to be making.

We could leave it at that, and conclude that Kant has really nothing to say about Hume's analysis of cause but only about his failure to se how far our ability to grasp and be aware of our experience depend upon the obedience of experience to causal laws. I was myself incline to leave it at that, until I began to pay close attention to a passage (A in the *Critique* from which emerges a new line of thought altogeth I number its sentences for purposes of reference; and, since I am tak the passage rather seriously, I should mention that the translati Kemp Smith's and—as almost always with this translator—is be reproach.

> (1) If we [said] that experience continually presents examples
> regularity among appearances and so affords abundant opportu
> abstracting the concept of cause, and at the same time of ve
> objective validity of such a concept, we should be overlo
> that the concept of cause can never arise in th
> this concept makes strict demand th
> that something else, B, follows
> *an absolutely universal*

we can find will have exactly *one* known exception each, then things are only one-fiftieth as bad. Strict universality is the ideal, but only as the maximum of something which it is desirable to have in as high a degree as possible.

In contrast with this, it is Kant's view that there is a great gulf between exceptionless universal statements and universal statements which have one counter-instance each; but that the difference between 'one counter-instance' and 'two counter-instances' or 'one hundred and seventeen counter-instances' is of no theoretical importance at all, because the real damage is done by the admission of a single counter-instance. This is because an 'explanation' of an event which appeals to a statement of the form 'Very nearly every time A occurs, B follows' *is not an explanation at all.* The production of such a statement might soothe someone into saying 'You have explained the event to me' or 'Now I know why it occurred' or 'Now I understand'; but the event would *not* in fact have been explained; the hearer would *not* know why; he would *not* understand. It is of the essence of the notions of *explanation*, of *understanding*, of something's being so *because* something else is so, of an event's being *made* to occur, that these are all expressible in terms of '*an absolutely universal rule*'. The flaw in Hume's position is that he cannot do justice to this fact. Hume's only reason for connecting 'explain', 'understand', 'because' and the rest with exceptionless universal statements is his *contingent* belief that there are exceptionless rules to be found and that any rule which has one exception can be made to have indefinitely many exceptions. Just because he thinks that the concept of causality can be analysed in purely empirical terms—and crucially in terms of regularity—Hume does not *and cannot* attach a fundamental philosophical importance to the difference between a rule which always holds and one which nearly always holds.

I wish now to bring out two connections between the notion of regulativeness and Kant's view that genuine explanations demand a strongly quantified science. One of them involves using Kant's view about strong quantification to clear up a difficulty about his handling of regulativeness; the other reverses the order and uses the notion of regulativeness as an aid to adjudicating between Kant and Hume in their conflict over strong quantification.

Firstly, let us return to the question of why, having proved that all graspable experience must have a fairly high degree of causal order, Kant claims to have proved that total determinism is strictly true.

There are two ways in which it is *prima facie* possible that a fairly orderly world should fall short of being totally causally determined. (1) It might manifest a causal order which is fully expressible in strongly quantified statements which do not claim all the territory—that is, which give no answers at all to some questions about what, in given fully described situations, will happen next. (2) It might manifest a causal order which is expressible in statements which give answers to every question about what will happen next, but only weakly quantified statements of this sort. If we think only in terms of strongly quantified science, then these two *prima facie* possibilities are the possibilities (1) that a world might admit only of an *uncomprehensive* science and (2) that a world might admit only of a *slightly inaccurate* science.

I have no reason to think that the first of these ever occurred to Kant as a possibility, but in the present context this oversight is not a damaging one. There could of course be an accurate but uncomprehensive science; but I doubt if there could be a partially ordered world the whole of whose order was expressible in an accurate, strongly quantified science—so that events which were not predictable by this science could not be brought under any generalisations at all, even weakly quantified ones. If I am right in thinking that this is not possible, then any world which was not totally determined would have to owe at least part of the incompleteness of its causal order to the second of the two possibilities, that is, to its admitting only of a slightly inaccurate science or a weakly quantified one.

Now, Kant's arguments for the second analogy all turn on such notions as those of *explanation*, *retrodiction*, *reasons* for believing, the way in which something *must* have happened, and so on. What his arguments show is that we cannot have graspable experience at all unless we have experience to which we can apply notions like these. But his complaints against Hume include (I have argued) the claim that we are not using these notions properly unless we are using them in connection with a true, strongly quantified science; and I have suggested that a fairly orderly world can escape being totally determined only if its science is either not-quite-true or weakly quantified. And *this*, I submit, is why Kant thinks that there is no gap between the notion of a world which is orderly enough for the notions of explanation, etc., to be applicable to it and that of a world which is totally determined; it therefore provides the reason for Kant's exaggerated description of what he has achieved in his arguments for the second analogy of experience. Q.E.D.

By speaking of ' exaggeration ', I commit myself to taking Hume's side against Kant on the issue of weakly *versus* strongly quantified science. I believe that most people find Kant's view here the initially attractive and plausible one, and I hope that this is so because I do not wish to take up space here trying to increase its attractiveness before showing it to be false.

Let K be a scientist whose working assumption about what kinds of causal laws can be found is the strictly determinist one which Kant thinks to be a *sine qua non* of scientific respectability. Let H be another scientist whose working assumption is that the world admits of a weakly, but not of a strongly, quantified science. I wish to examine the ways in their respective assumptions affect the behaviour of K and H as scientists, in order to see whether Kant is right in withholding from H the cachet of intellectual respectability which he confers upon K.

H thinks that some events are causally inexplicable, while K denies this. Suppose them to be confronted by an event for whose occurrence they have no explanation. It is not an event upon which received science is *silent*, but one whose occurrence is *in conflict with* some hypothesis which has hitherto been a part of received science. A short description of what then happens would be as follows: K says that the hypothesis in question must be dropped and a replacement for it sought, and he maintains this in face of persistent failure to replace the hypothesis; H on the other hand regards it as possible that the challenged hypothesis is the best hypothesis of its kind of which the world admits, and therefore that the event whose occurrence conflicts with it is simply an illegality which cannot be coped with in science at all.

K stands a chance of discomfiting H—by finding an explanation for the event which H has conjectured to be inexplicable—while H has no comparable way of discomfiting K. Those of us who were brought up to think of vulnerability as a virtue may well regard this as a point in favour of H's position. But there is a reason which might be given for awarding the honours the other way, namely that, although H and K both make acts of faith, it is K who makes the optimistic, industry-urging act of faith. To give this reason for favouring K's strict determinism is to appeal to its alleged merit as salutary advice, that is to its alleged acceptability as a regulative principle. More than this is needed to justify Kant's position, for he says that a belief in illegalities is not just mildly bad for the scientist who holds it but is radically incompatible with the rational understanding of the world. Nevertheless, I wish to discuss the weaker claim for the regulative virtue of strict determinism, as a discussion of this will give us all we need for a refutation of Kant's stronger claim that strict determinism is not only salutary for the scientist but is mandatory upon him.

The suggestion before us, then, is that when K insists that there is an explanation to be found for the as-yet-unexplained event, he is saying something which is healthier, because less conducive to complacency, than H's conjecture that the event is an illegality. The short reply to this is that it is a libel on H to assume that he is not prepared to test his conjecture; and to test it he will to have do exactly what K does, namely to search as hard as he can for an explanation for the event in question. At any level of science, the behaviour of the honest friends of an hypothesis is the same as that of its earnest enemies.

'Still', it may be said, 'H will eventually give up the search for an explanation. He lacks K's ultimate commitment to a belief in the total intelligibility of the world, and this failure of faith will sooner or later sap his will and lead him to abandon the pursuit.' This is correct; but *it is not a difference between H and K.*

It must be remembered that the problem concerns a *single* event, not a large *class* of events of a certain kind. H and K have adopted a hypothesis about (say) sulphates and then discovered not that it fails for (all samples of) copper sulphate, but that it failed for *one particular sample of copper sulphate on one particular Wednesday morning.* If their difficulty arises from an experiment which can be reproduced at will, and always with the same result, then H and K do have a pair of strictly universal statements which cover the known facts: one about sulphates other than copper-sulphate-under-conditions-C, and one about copper-sulphate-under-conditions-C. Much time may be spent in trying to derive both hypotheses from some more general hypothesis; but even if they are not as general as H and K might wish they are nevertheless 'strictly universal' and so suffice to ward off our Kantian problem. Our problem arises only when a hypothesis is challenged by a single datable, locatable, unrepeatable experiment, not when it is challenged by a kind of experiment of which there are many instances.

There is, I think, universal agreement amongst scientists and philosophers of science that unrepeatable experiments *do not count* against well-corroborated hypotheses. So, when H—faced with the stubborn failure of his attempts to repeat the experiment—says that the challenged hypothesis ought to be retained and the challenging experiment relegated to limbo, he is only saying what K too will say if he does not wish to make an ass of himself in the eyes of the scientific community. The only difference between them is that H can give a better account than K can of why he refuses to give up a hypothesis which is challenged by an unrepeatable experiment. For H can say that he is acting on the conjecture that the experiment was an illegality and that the challenged hypothesis has that 'true with very few exceptions' status which is the most that we can hope for in science. K of course cannot say this; but what *can* he say?

He may say that he does regard the hypothesis as refuted, but that he continues to treat it as though it were unrefuted because he has nothing to put in its place. But if he says this, he will find himself estranged from the rest of the scientific community: not because he is so lax as to treat a hypothesis as true which he believes to have been refuted, but because he is so silly as to believe a hypothesis to have been refuted by an unrepeatable experiment.

I shall suppose him, then, to say whatever the other scientists and the philosophers of science say about the negligibility of unrepeatable experiments. The trouble is that most of them say nothing; and those who do pronounce on the subject are quite unclear about the particular point which I am raising. However, if we can see clearly what are the *possible* positions on this matter, we can afford to ignore the question of which philosophers have in fact taken which position. Stripping the issue to its bare essentials, we can say that if a well-corroborated hypothesis of the form '$(x)(Fx \supset Gx)$' is challenged by the production of an experimental report of the form '$Fa \,.\, Ha \,.\, \sim Ga$', and all attempts to repeat the experiment described therein meet with failure, there are just three possibilities: (1) To say that the hypothesis is refuted by the single experiment; (2) To say that the experimental report is not true; and (3) To say that the experimental report is not true but the hypothesis is unrefuted—this is what H says, and it commits anyone who says it to reading the hypothesis as weakly quantified.

Option (1) is eccentric and uninteresting, and I shall discuss it no further; the important and interesting clash is that between options (2) and (3). I do not think it has been generally recognised that the orthodox view about unrepeatable experiments confronts us with the disjunction of (2) and (3), but there are signs in the literature that this has been half-recognised: some writers speak of unrepeatable experiments in terms appropriate to the adoption of (2), and the only reason

I can think of for adopting (2) as necessarily the right account is a Kantian belief that to adopt (3) is in some way to let the side down. The trouble with option (2)—that is, with saying that the experimental report is untrue in some relevant respect—is that there *could be* cases in which it was entirely implausible. It does not matter whether there have been such cases: their mere possibility is sufficient to show that the *insistence* upon a strongly quantified science, which debars one from *ever* taking option (3) when dealing with an unrepeatable experiment, is irrational and unsatisfactory.

In saying that there could be cases in respect of which option (2) was implausible, I am not calling attention merely to those cases where we have no reason to suspect the integrity and basic professional competence of the author of the experimental report. A report may be deemed untrue without the honesty or basic competence of its author being called into question, if the doubts do not concern those parts of the report which say what apparatus was used, what readings were taken, what kind of graft was made, etc., but rather those parts which say that the sample used was pure, that the meteorological conditions did not vary during the experiment, that adequate safeguards were taken to prevent cross-pollination. Claims of this sort will always be needed if the reported experiment is to be made relevant to a received hypothesis, and a doubt which is cast upon any such claim in an experimental report is a doubt as to the truth of the report *in so far as the report says that something happened which was in conflict with a received hypothesis.* It is in connection with this sort of doubt, I think, that it has become customary to dismiss unrepeatable experiments as involving some sort of ' experimental error '.

But it is just dogmatism to say that wherever an experiment tur out to be unrepeatable it is *ipso facto* reasonable to say that it involved some ' experimental error ' of this sort. If the claim that there was an experimental error has no backing except for the fact that the experiment has turned out to be unrepeatable, then the imputation of experimental error is just a face-saving, word-spinning device which has much less to recommend it than has the alternative of saying that the hypothesis under challenge must be taken as weakly quantified. The impression is sometimes given in the literature that it is always reasonable to take option (2) in connection with an unrepeatable experiment, because repeatability is a necessary condition of the experiment's counting as an ' objective ' happening, or of its not counting as ' occult ' or ' chimerical '. The picture which this evokes is that of a scientist ending his experimental report with something like ' . . . and then I saw a blue flash; no-one else was in the laboratory at the time, but I swear that I saw it '. But of course the circumstances of the experiment do not have to be like this at all: there can be witnesses, research assistants, photographs, recorded running commentaries—a whole host of sources of evidence that the experiment did take place as reported. In these circumstances, it would be merely arbitrary to insist that nevertheless there *was* an ' experimental error ' in the relevant sense, or that what happened was ' chimerical '; but it would not be arbitrary to dismiss the experiment as negligible on the grounds of its unrepeatability. In such a case, the only person who could give a sensible account of *why* the experiment was negligible would be the one who—like my scientist H—does not insist upon a strongly quantified science.

In conclusion, then, I maintain that a preparedness to accept a weakly quantified science is not only permissible but is mandatory upon any scientist who wishes to be able to cope sensibly with a really well-attested but unrepeatable experiment, if one should occur. Kant's insistence upon ' absolutely universal rules ' is not just the contradictory, but a contrary, of the truth.

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PROOFS AND REFUTATIONS (II) *

I. LAKATOS

(c) *Improving the conjecture by exception-barring methods. Piecemeal exclusions. Strategic withdrawal or playing for safety*

BETA: I suppose, sir, you are going to explain your puzzling remarks. But, with all apologies for my impatience, I must get this off my chest.

TEACHER: Go on.

(ALPHA *re-enters.*)

BETA: I find some aspects of Delta's arguments silly, but I have come to believe that there is a reasonable kernel to them. It now seems to me that no conjecture is generally valid, but only valid in a certain restricted domain that excludes the *exceptions.* I am against dubbing these exceptions ' monsters ' or ' pathological cases '. That would amount to the methodological decision not to consider these a interesting *examples* in their own right, worthy of a separate investigation. But I am also against the term '*counterexample*'; it rightly admits them as examples on a par with the supporting examples, but somehow paints them in war-colours, so that, like Gamma, one panics when facing them, and is tempted to abandon beautiful and ingenious proofs altogether. No: they are just *exceptions.*

SIGMA: I could not agree more. The term ' counterexample ' has an agressive touch and offends those who have invented the proofs. ' Exception ' is the right expression. ' There are three sorts of mathematical propositions:

' 1. Those which are always true and to which there are neither restrictions nor exceptions, e.g. the angle sum of all plane triangles is always equal to two right angles.

' 2. Those which rest on some false principle and so cannot be admitted in any way.

' 3. Those which, although they hinge on true principles, nevertheless admit restrictions or exceptions in certain cases. . . .'

EPSILON : What?

SIGMA: '. . . One should not confuse false theorems with theorems subject to some restriction.'[1] As the proverb says: *The exception proves the rule.*

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* Part I appeared in the previous number.

EPSILON (*to* KAPPA): Who is this muddlehead? He should learn something about logic.

KAPPA (*to* EPSILON): And about non-Euclidean plane triangles.

DELTA: I find it embarrassing to have to predict that in this discussion Alpha and I shall probably be on the same side. We both argued on the basis of a proposition's being either true or false and disagreed only on whether the Euler theorem, in particular, is true or false. But Sigma wants us to admit a third category of propositions that are 'in principle' true but 'admit exceptions in certain cases'. To agree to a peaceful coexistence of theorems and exceptions means to yield to confusion and chaos in mathematics.

ALPHA: *D'accord.*

ETA: I did not want to interfere with the brilliant argumentation of Delta, but now I think it may be profitable if I briefly explain the story of *my* intellectual development. In my schooldays I became—as you would put it—a monsterbarrer, not as a defence against Alpha-types but as a defence against Sigma-types. I remember reading in a periodical about the Euler theorem: 'Brilliant mathematicians have put forward proofs of the general validity of the theorem. Nevertheless it suffers exceptions . . . it is necessary to draw attention to these exceptions since even recent authors do not always recognise them explicitly.'[2] This paper was not an isolated exercise in diplomacy. 'Although in geometry textbooks and lectures it is always pointed out that Euler's beautiful theorem $V+F=E+2$ is subject to "restriction" in some cases, or "does not seem to be valid", one does not learn the real reason for these exceptions.'[3] Now I looked at the

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[1] Bérard [1818-19], p. 347 and p. 349

[2] Hessel [1832], p. 13. Hessel rediscovered Lhuilier's 'exceptions' in 1832. Just after submitting his manuscript he came across Lhuilier's [1812-13]. He nevertheless decided not to withdraw the paper, most of whose results thus turned out to have already been published, because he thought that the point should be driven home to the 'recent authors' ignoring these exceptions. One of these authors, by the way, happened to be the Editor of the Journal to which Hessel submitted the paper: A. L. Crelle. In his [1826-27] textbook he 'proved' that Euler's theorem was true for *all* polyhedra (Vol. II, pp. 668-671).

[3] Matthiessen ([1863], p. 449). Matthiessen refers here to Heis and Eschweiler's *Lehrbuch der Geometrie* and to Grunert's *Lehrbuch der Stereometrie*. Matthiessen however does not solve the problem—like Eta—by monsterbarring, but—like Rho—by monster-adjustment (cf. footnote 1, p. 135).

'exceptions' very carefully and I came to the conclusion that they do not comply with the true definition of the entities in question. So the proof and the theorem can be reinstated and the chaotic coexistence of theorems and exceptions vanishes.

ALPHA: Sigma's chaotic position may serve as an explanation for your monsterbarring, but not as an excuse, let alone a justification. Why not eliminate the chaos by accepting the credentials of the counterexample and rejecting the 'theorem' and the 'proof'?

ETA: Why should I reject the proof? I cannot see anything wrong with it. Can you? My monsterbarring seems more rational to me than your proof-barring.

TEACHER: This debate showed that monsterbarring may get a more sympathetic audience when it stems from Eta's dilemma. But let us come back to Beta and Sigma. It was Beta who rechristened the counterexamples exceptions. Sigma agreed with Beta. . . .

BETA: I am glad that Sigma agreed with me, but I am afraid that I cannot agree with him. There are certainly three types of propositions: true ones, hopelessly false ones and hopefully false ones. This last type can be improved into true propositions by adding a restrictive clause which states the exceptions. I never 'attribute to formulae an undetermined domain of validity. In reality most of the formulae are true only if certain conditions are fulfilled. By determining these conditions and, of course, pinning down precisely the meaning of the terms I use, I make all uncertainty disappear.'[1] So, as you see, I do not advocate any sort of peaceful coexistence between unimproved formulae and exceptions. I improve my formulae and turn them into *perfect* ones, like those in Sigma's first class. This means that I *accept* the method of monsterbarring in so far as it serves for finding *the domain of validity of the original conjecture*; I *reject* it in so far as it functions as a linguistic trick for rescuing 'nice' theorems by restrictive concepts. These two functions of Delta's method should be kept separate. I should like to baptise *my* method, which is characterised by the first of these functions only, '*the exception-barring method*'. I shall use it to determine precisely the domain in which the Euler conjecture holds.

TEACHER: What is the 'precisely determined domain' of Eulerian polyhedra you promised? What is your 'perfect formula'?

BETA: *For all polyhedra that have no cavities (like the pair of nested cubes) and tunnels (like the picture-frame),* $V - E + F = 2$.

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[1] This is from Cauchy's introduction to his celebrated [1821].

TEACHER: Are you sure?

BETA: Yes, I am sure.

TEACHER: What about the twintetrahedra?

BETA: I am sorry. *For all polyhedra that have no cavities, tunnels or 'multiple structure'*, $V - E + F = 2$.[1]

TEACHER: I see. I agree with your policy of improving the conjecture instead of just taking or leaving it. I prefer it both to the method of monsterbarring and to that of surrender. However, I have two objections. *First* I contend that your claim that your method not only improves, but 'perfects' the conjecture, that it 'renders it strictly correct', that 'it makes all uncertainties disappear' is untenable. The *adhocness* of your method destroys its chance of achieving certainty.

BETA: Indeed?

TEACHER: You must admit that each new version of your conjecture is only an *ad hoc* elimination of a counterexample which has just cropped up. When you stumble upon nested cubes you exclude polyhedra with *cavities*. When you happen to notice a picture-frame, you exclude polyhedra with *tunnels*. I appreciate your open and observant mind; to take notice of these exceptions is all very well, but I think it would be worth while to inject some method into your blind groping for 'exceptions'. It is good to admit that 'All polyhedra are Eulerian' is only a conjecture. But why give 'All polyhedra without cavities, tunnels and what not are Eulerian' the status of a theorem that is not conjectural any more? How can you be sure that you have enumerated *all* exceptions?

BETA: Can you give one that I did not take into account?

ALPHA: What about my urchin?

GAMMA: And my cylinder?

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[1] Lhuilier and Gergonne seem to have been sure that Lhuilier's list had enumerated all the exceptions. We read in the introduction to this part of the paper: 'One will easily be convinced that Euler's Theorem is true in general, for all polyhedra, whether they are convex or not, except for those instances that will be specified . . .' (Lhuilier [1812-13], p. 177). Then we read again in Gergonne's comment: '. . . the specified exceptions which seem to be the only ones that can occur. . . .' (ibid. p. 188). *But in fact Lhuilier missed the twintetrahedra, which were only noticed twenty years later by Hessel* ([1832]). That some leading mathematicians, even mathematicians with a lively interest in methodology like Gergonne, could believe that one could rely upon the exception-barring method, is noteworthy. The belief is analogous to the 'method of division' in inductive logic, according to which there can be a complete enumeration of possible explanations of a phenomenon, and therefore the method of *experimentum crucis*, which eliminates all but one, *proves* this last one.

TEACHER: I do not even need a concrete new 'exception' for my argument. My argument was for the *possibility* of further exceptions.

BETA: You may well be right. One should not just shift one's position whenever a new counterexample turns up. One should not say: 'If no exception occur from phenomena, the conclusion may be pronounced generally. But if at any time afterwards any exception should occur, it may then begin to be pronounced with such exceptions as occur.'[1] Let me think. We first guessed that for *all* polyhedra $V-E+F=2$, because we found it to be true for cubes, octahedra, pyramids, and prisms. We certainly cannot accept 'this miserable way of inferring from the special to the general'.[2] No wonder exceptions cropped up; it is rather surprising that many more were not found much earlier. To my mind this was because we were mostly occupied with *convex* polyhedra. As soon as other polyhedra entered, our generalisations did not work any more.[3] So instead of barring exceptions piecemeal, I shall draw the borderline modestly, but safely: *All convex polyhedra are Eulerian.*[4] And I hope you will grant that this has nothing conjectural about it: that it is a theorem.

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[1] I. Newton [1717], p. 380

[2] Abel [1826]. His criticism seems to be directed against Eulerian inductivism.

[3] This too is paraphrased from the quoted letter, in which Abel was concerned to eliminate the exceptions to general 'theorems' about functions and thereby establish absolute rigour. The original text (including the previous quotation) is this: 'In Higher Analysis very few propositions are proved with definitive rigour. *One finds everywhere the miserable way of inferring from the special to the general*, and it is a marvel that such procedure leads only rarely to what are called paradoxes. It is really very interesting to look for the reason. In my opinion the reason is to be found in the fact that *analysts have been mostly occupied with functions that can be expressed as power series. As soon as other functions enter—which certainly is rarely the case—one does not get on any more* and as soon as one starts drawing false conclusions, an infinite multitude of mistakes will follow, all supporting each other . . .' (my italics). Poinsot discovered that inductive generalisations 'often' break down in the theory of polyhedra, just as in number theory: 'Most properties are individual and do not obey any general laws' ([1809], § 45). The intriguing characteristic of this caution towards induction is that it puts down its occasional breakdown to the fact that the universe (of facts, numbers, polyhedra) of course contains miraculous exceptions.

[4] This again is very much in keeping with Abel's method. In the same way Abel restricted the domain of suspect theorems about functions to power-series. In the story of the Euler conjecture this restriction to convex polyhedra was fairly common Legendre, for instance, after giving his rather general definition of polyhedron (cf. footnote 1 p. 16), presents a proof which on the one hand certainly does not apply to all his general polyhedra, but on the other hand applies to more than convex ones.

GAMMA: What about my cylinder? It is convex!

BETA: It is a joke!

TEACHER: Let us forget about the cylinder for the moment. We can offer some criticism even without the cylinder. In this new, modified version of the exception-barring method, which, Beta devised so briskly in answer to my criticism, piecemeal withdrawal has been replaced by a strategic retreat into a domain hoped to be a stronghold of the conjecture. You are playing for safety. But are you as safe as you claim to be? You still have no guarantee that there will not be any exceptions inside your stronghold. Besides, there is the opposite danger. Could you have withdrawn too radically, leaving lots of Eulerian polyhedra outside the walls? Our original conjecture might have been an overstatement, but your 'perfected' thesis looks to me very much like an understatement; yet you still cannot be sure that it is not an overstatement as well.

But I should also like to put forward my *second* objection: your argument forgets about the proof; in guessing the domain of validity of the conjecture, you do not seem to need the proof at all. Surely you do not believe that proofs are redundant?

BETA: I have never said that.

TEACHER: No, you did not. But you discovered that our proof did not prove our original conjecture. Does it prove your improved conjecture? Tell me.

BETA: Well . . .[1]

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evertheless, in an additional note, in fine print (an afterthought after having stumbled on exceptions never stated?), he withdraws, modestly but safely, to convex polyhedra ([1809], pp. 161, 164, 228).

[1] Many working mathematicians are puzzled about what proofs are for if they do not prove. On the one hand they know from experience that proofs are fallible but on the other hand they know from their dogmatist indoctrination that *genuine* proofs must be infallible. *Applied mathematicians* usually solve this dilemma by a shamefaced but firm belief that the proofs of the *pure mathematicians* are 'complete', and so *really* prove. Pure mathematicians, however, know better—they have such respect only for the 'complete proofs' of *logicians*. If asked what is then the use, the function, of their 'incomplete proofs', most of them are at a loss. For instance, G. H. Hardy had a great respect for the logicians' demand for formal proofs, but when he wanted to characterise mathematical proof 'as we working mathematicians are familiar with it', he did it in the following way: 'There is strictly speaking no such thing as mathematical proof; we can, in the last analysis, do nothing but point; . . . proofs are what Littlewood and I call *gas*, rhetorical flourishes designed to affect psychology, pictures on the board in the lecture, devices to stimulate the imagination of pupils' ([1928], p. 18). R. L. Wilder thinks that a proof is 'only a testing process

ETA: Thank you, sir, for this argument. Beta's embarrassment clearly displays the superiority of the defamed monsterbarring method. For we say that the proof proves what it has set out to prove and our answer is unequivocal. We do not allow wayward counterexamples to destroy respectable proofs at liberty, even if they are disguised as meek ' exceptions '.

BETA: I do not find it embarrassing at all that I have to elaborate, improve, and—excuse me, sir—*perfect* my methodology on the stimulus of criticism. My answer is this. I reject the original conjecture as false because there are exceptions to it. I also reject the proof because the same exceptions are exceptions to at least one of the lemmas. (In your terminology this would be: a global counterexample is necessarily also a local counterexample.) Alpha would stop at this point since refutations seem to satisfy his intellectual needs completely. But I go on. By suitably restricting *both* conjecture and proof to the proper domain, I perfect the *conjecture* which will now be *true*, and perfect the basically sound *proof* which will now be *rigorous* and will obviously contain no more false lemmas. For instance we saw that not all polyhedra can be stretched flat onto a plane after having a face removed. But all *convex* polyhedra can. I can rightly call my perfected and rigorously proved conjecture a *theorem*. I state it again ' *All convex polyhedra are Eulerian.*' For convex polyhedra all the lemmas will be manifestly true and the proof, which was not rigorous in its false generality, will be rigorous for the restricted domain of convex polyhedra. So, sir, I have answered your question.

TEACHER: So the lemmas, which once looked manifestly true befo[illegible] the exception was discovered, will again look manifestly true . . . . until the discovery of the next exception. You admit that ' All polyhedra are Eulerian ' was guesswork; you admitted just now that ' All polyhedra without cavities and tunnels are Eulerian ' was also guesswork; why not admit that ' All convex polyhedra are Eulerian ' is guesswork once again!

BETA: Not '*guesswork*' this time, but *insight*!

TEACHER: I abhor your pretentious ' insight '. I respect conscious *guessing*, because it comes from the best human qualities: courage and modesty.

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that we apply to suggestions of our intuition' ([1944], p. 318). G. Pólya points out that proofs, even if incomplete, establish connections between mathematical facts and this helps us to keep them in our memory: proofs yield a mnemotechnic system ([1945], pp. 190-191).

BETA: I proposed a theorem: 'All convex polyhedra are Eulerian.' You offered only a sermon against it. Could you offer a counterexample?

TEACHER: You cannot know that I shall not. You *improved* the original conjecture, but you cannot claim to have *perfected* the conjecture, to have achieved perfect rigour in your proof.

BETA: Can *you*?

TEACHER: I cannot either. But I think that my method of improving conjectures will be an improvement on yours for I shall establish a unity, a real interaction, between proofs and counterexamples.

BETA: I am ready to learn.

(*d*) *The method of monster-adjustment*

RHO: Sir, may I get a few words in edgeways?

TEACHER: By all means.

RHO: I agree that we should reject Delta's monster-barring as a general methodological approach, for it doesn't really take 'monsters' seriously. Beta doesn't take his 'exceptions' seriously either, for he merely lists them and then retreats into a safe domain. Thus both these methods are interested only in a limited, privileged field. *My* method does not practise discrimination. I can show that 'on closer examination the exceptions turn out to be only apparent and the Euler theorem retains its validity even for the alleged exceptions.'[1]

TEACHER: Really?

ALPHA: How can my counterexample 3, the 'urchin' (Fig. 5), be an ordinary Eulerian polyhedron? It has 12 star-pentagonal faces. . . .

RHO: I don't see any 'star-pentagons'. Don't you see that in actual fact this polyhedron has ordinary *triangular* faces? There are 60 of them. It also has 90 edges and 32 vertices. Its 'Euler characteristic' is 2.[2] The 12 'star-pentagons', their 30 'edges' and 12 'vertices', yielding the 'characteristic' -6, are only your fancy. Monsters don't exist, only monstrous interpretations. One has to purge one's mind from perverted illusions, one has to learn how to see and how to define correctly what one sees. My method is therapeutic: where you—erroneously—'see' a counterexample, I teach you how to recognise—correctly—an example. I adjust your monstrous vision. . . .[1]

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[1] L. Matthiessen [1863].

[2] The argument that the 'urchin' is 'really' an ordinary, prosaic Eulerian polyhedron with 60 triangular faces, 90 edges and 32 vertices —'*un hexacontaèdre sans épithète*'—was put forward by the staunch champion of the infallibility of the Euler theorem, E. de Jonquières ([1890*a*], p. 115). The idea of interpreting non-Eulerian star-polyhedra as triangular Eulerian polyhedra does not however stem from Jonquières but has a dramatic story (cf. footnote 2, p. 128).

ALPHA: Sir, please explain *your* method, before Rho brainwashes us.[2]

TEACHER: Let him go on.

RHO: I have made my point.

GAMMA: Could you enlarge on your criticism of Delta's method? Both of you exorcised 'monsters'. . . .

RHO: Delta was taken in by your hallucinations. He agreed that your 'urchin' has 12 faces, 30 edges and 12 vertices, and is non-Eulerian. His thesis was that it is not a polyhedron either. But he erred on both counts. Your 'urchin' *is* a polyhedron and *is* Eulerian. But its star-polyhedral concept was a misinterpretation. If you don't mind, it is not the imprint of the urchin on a healthy, pure mind, but its distorted imprint on a sick mind, twisting in pain.[1]

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[1] Nothing is more characteristic of a dogmatist epistemology than its theory of error. For if some truths are manifest, one must explain how anyone can be mistaken about them, in other words, why the truths are not manifest to everybody. According to its particular theory of error, each dogmatist epistemology offers its particular therapeutics to purge minds from error. Cf. Popper [1963], Introduction.

[2] Poinsot certainly was brainwashed some time between 1809 and 1858. It was Poinsot who rediscovered star-polyhedra, first analysed them from the point of view of Euleriannes, and stated that some of them, like our small stellated dodecahedron, do not comply with Euler's formula ([1809]). Now this very Poinsot states categorically in his [1858] that Euler's formula 'is not only true for convex polyhedra, but for any polyhedron whatsoever, including star-polyhedra' (p. 67—Poinsot uses the term *polyèdres d'espèce supérieure* for star polyhedra). The contradiction is obvious. What is the explanation? What happened to the star-polyhedral *counterexamples*? The clue is in the first casual-looking sentence of the paper: 'One can reduce the whole theory of polyhedra to the theory of polyhedra with *triangular* faces'. That is, Poinsot-Alpha was brainwashed and turned into Poinsot-Rho: now he sees only triangles where he previously saw star-polygons: now he sees only examples where he previously saw counterexamples. The self-criticism had to be surreptitious, cryptic, because in scientific tradition there are no patterns available for articulating such volte-faces. One also wonders, did he ever come across ring-shaped faces and if so, did he knowingly reinterpret them with his triangular vision?

The change of vision need not always operate in the same direction. For example J. C. Becker in his [1869]—fascinated by the new conceptual framework of simply- and multiply-connected domains (Riemann [1851])—allowed for ring-shaped polygons but remained blind to star-polygons (p. 66). Five years after this paper—in which he claimed to have brought the problem to a 'definitive' solution—he broadened his vision and recognised star-polygonal and star-polyhedral patterns where he previously saw only triangles and triangular polyhedra ([1874]).

KAPPA: But how can you distinguish healthy minds from sick ones, rational from monstrous interpretations?[2]

RHO: What puzzles *me* is how you can mix them up!

SIGMA: Do you really think, Rho, that Alpha never noticed that his 'urchin' might be interpreted as a triangular polyhedron? Of course it might. But a closer look reveals that 'these triangles always lie in fives in the same plane and surround a regular pentagon hiding—like their heart—behind a solid angle. Now the five regular triangles together with the inner heart—the regular pentagon—form a so-called "pentagramma" that according to Theophrastus Paracelsus was the sign of health. . . .'[3]

RHO: Superstition!

SIGMA: And so for the *healthy* mind the secret of the urchin will be revealed: that it is a new, hitherto undreamt-of regular body, with regular faces an[illegible]ual solid angles, the beautiful symmetry of which might reveal to us the secrets of universal harmony. . . .[4]

ALPHA: Thank you, Sigma, for your defence which again convinces me that opponents are less embarrassing than allies. Of course my polyhedral figure can be interpreted either as a triangular polyhedron or as a star-polyhedron. I am willing to admit both interpretations on a par. . . .

KAPPA: Are you?

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[1] This is part of a Stoic theory of error, attributed to Chrysippos (cf. Aetius [c. 150], IV.12.4; also Sextus Empiricus [c. 190], I. 249).

According to the Stoics the 'urchin' would be part of external reality, which produces an imprint upon the soul: the *phantasia* or *visum*. A wise man will not give uncritical assent (*synkatathesis* or *adsensus*) to a *phantasia* unless it matures into a clear and distinct idea (*phantasia katalēptikē* or *comprehensio*), which it cannot do if it is false. The system of clear and distinct ideas forms science (*epistēmē*). In our case the imprint of the 'urchin' on Alpha's mind would be the small stellated dodecahedron, while on Rho's mind it would be the triangular hexacontaeder. Rho would claim that Alpha's star-polyhedral vision cannot possibly mature into a clear and distinct idea, obviously since it would upset the 'proved' Euler formula. Thus the star-polyhedral interpretation would fail and the 'only' alternative to it, namely the triangular interpretation, would become clear and distinct.

[2] This is a standard Sceptic criticism of the Stoic claim that they can distinguish *phantasia* from *phantasia katalēptikē* (e.g. Sextus Empiricus [c. 190], I. 405).

[3] Kepler [1619], Lib. II. Propositio XXVI.

[4] This is a fair exposition of Kepler's view.

Delta: But surely one of them is the *true* interpretation!

Alpha: I am willing to admit both interpretations on a par, but one of them will certainly be a global counterexample to Euler's conjecture. Why admit only the interpretation that is ' well-adjusted ' to Rho's preconceptions? Anyway, Sir, will you now explain *your* method?

(e) *Improving the conjecture by the method of lemma-incorporation. Proof-generated theorem versus naive conjecture*

Teacher: Let us return to the picture-frame. I for one recognise it as a genuine global counterexample to the Euler conjecture, as well as a genuine local counterexample to the first lemma of my proof.

Gamma: Excuse me, Sir—but how does the picture-frame refute the first lemma?

Teacher: First remove a face and then try to stretch it flat on the blackboard. You will *not* succeed.

Alpha: To help your imagination, I will tell you that those and only those polyhedra which you can inflate into a sphere have the property that, after a face is removed, you can stretch the remaining part onto a plane.

It is obvious that such a ' spherical ' polyhedron is stretchable onto a plane after a face has been cut out; and vice versa it is equally obvious that, if a polyhedron minus a face is stretchable onto a plane, then you can bend it into a round vase which you can then cover with the missing face, thus getting a spherical polyhedron. But our picture frame can never be inflated into a sphere; but only into a torus.

Teacher: Good. Now, unlike Delta, I accept this picture-frame as a criticism of the conjecture. I therefore discard the conjecture in its original form as false, but I immediately put forward a modified, restricted version, namely this: the Descartes-Euler conjecture holds good for ' simple ' polyhedra, i.e. for those which, after having had a face removed, can be stretched onto a plane. Thus we have rescued some of the original hypothesis. We have: *The Euler characteristic of a simple polyhedron is 2.* This thesis will not be falsified by the nested cube, by the twin-tetrahedra, or by star-polyhedra—for none of these is ' simple.'

So while the exception-barring method restricted both the domain of the main conjecture and of the guilty lemma to a common domain of safety, thereby accepting the counterexample as criticism both of the main conjecture and of the proof, my method of lemma-incorporation upholds the proof but reduces the domain of the main conjecture to the very domain of the guilty lemma. Or, while a counterexample which is both global and local made the exception-barrer revise both the lemmas and the original conjecture, it makes me revise the original conjecture, but not the lemmas. Do you understand?

ALPHA: Yes, I think I do. To show that I understand, I shall refute you.[1]

TEACHER: My method or my improved conjecture?

ALPHA: Your improved conjecture.

TEACHER: Then you may still not understand my method. But let us have your counterexample.

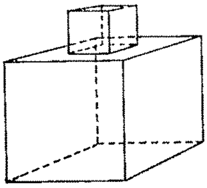

FIG. 12

ALPHA: Consider a cube with a smaller cube sitting on top of it (Fig. 12). This complies with all our definitions—Def. 0, 1, 2, 3, 3a, 4—so it is a genuine polyhedron. And it is ' simple ', in that it can be tretched on to the plane. Thus, according to your modified con- ecture, its Euler characteristic should be 2. Nonetheless it has 16 ertices, 24 edges and 11 faces, and its Euler characteristic is 16 — 24 11 = 3. It is a global counterexample to your improved con- cture and, by the way, also to Beta's first ' exception-barring ' theorem. is polyhedron, in spite of having no cavities, tunnels or ' multiple structure ', is *not* Eulerian.

DELTA: Let us call this crested cube *Counterexample 6*.[2]

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[1] I recall Karl Popper distinguishing three levels of understanding. The lowest was the pleasant feeling of having grasped the argument. The medium level was when one could repeat it. The top level was when one could refute it.

[2] *Counterexample 6* was noticed by Lhuilier ([1812-13], p. 186); Gergonne for once admits the novelty of his discovery! But almost fifty years later Poinsot had not heard of it [1858] while Matthiessen [1863] and, eighty years later, de Jonquières [1890*b*] treated it as a monster. (Cf. footnotes 2, p. 128, 1, p. 135.) Primitive exception-barrers of the nineteenth century listed it as a curiosity together with other ptions: ' As an example one is usually shown the case of a three sided pyramid ched to a face of a tetrahedron so that no edges of the former coincide with an ge of the latter. " Oddly enough, in this case $V - E + F = 3$ " is what is written

TEACHER: You have falsified my improved conjecture, but you have *not* destroyed my method of improvement. I shall re-examine the proof, and see why it broke down over your polyhedron. There must be another false lemma in the proof.

BETA: Of course there is. I have always suspected the second lemma. It presupposes that in the triangulating process, by drawing a new diagonal edge, you always increase by one the number of edges and of faces. This is false. If we look at the plane network of our crested polyhedron, we shall find a ring-shaped face (Fig. 13a). In this case no single diagonal edge will increase the number of faces (Fig. 13b): we need an increase of two edges to increase the number of faces by one (Fig. 13c).

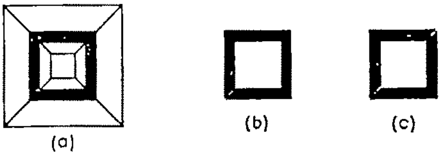

FIG. 13

TEACHER: My congratulations. I certainly must restrict our conjecture further. . . .

BETA: I know what you are going to do. You are going to say that '*Simple polyhedra with triangular faces are Eulerian*'. You will take triangulation for granted; and you will turn this lemma again into a condition.

TEACHER: No, you are mistaken. Before I point out your mistak concretely, let me enlarge upon my comment on your method exception-barring. When you restrict your conjecture to a 'saf domain, you do not examine the proof properly, and, in fact, you not need to for your purpose. The casual statement that in your restricted domain all the lemmas will be true whatever they are, is

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in my college notebook. And that ended the matter' (Matthiessen [1863], p. 449). Modern mathematicians tend to forget about ring-shaped faces, which may be irrelevant for the classification of manifolds but can become relevant in other contexts. H. Steinhaus says in his [1960]: 'Let us divide the globe into F countries (we shall consider *seas* and *oceans* as land). Then we shall have $V + F = E + 2$, whatever the political situation may be' (p. 273). But one wonders whether Steinhaus would destroy West Berlin or San Marino simply because their existence refutes Euler's theorem. (Though of course he may prevent seas like the Baikal from falling completely in one country by defining them as *lakes*, since he has said that only seas and oceans are to be considered as land.)

enough for your purpose. But this is not enough for mine. I build the very same lemma which was refuted by the counterexample *into* the conjecture, so that I have to spot it and formulate it as precisely as possible, on the basis of a careful analysis of the proof. The refuted lemmas thus will be incorporated in my improved conjecture. Your method does not force you to give a painstaking *elaboration of the proof*, since the proof does not appear in your improved conjecture, as it does in mine. Now I return to your present suggestion. The lemma which was falsified by the ring-shaped face was not—as you seem to think—that '*all faces are triangular*' but that '*any face dissected by a diagonal edge falls into two pieces*'. It is *this* lemma which I turn into a condition. Calling the faces which satisfy it 'simply-connected', I can offer a second improvement on my original conjecture: '*For a simple polyhedron, with all its faces simply-connected,* $V - E + F = 2$.' The reason for your rash mis-statement was that your method did not teach you careful proof-analysis. Proof-analysis is sometimes trivial, but sometimes very difficult indeed.

BETA: I see your point. I should also add a self-critical note to your comment, for it seems to me to reveal a whole continuum of exception-barring attitudes. The worst merely bars some exceptions without looking at the proof at all. Hence the mystification when we ave the proof on the one hand and the exceptions on the other. In e mind of such primitive exception-barrers, the proof and the ceptions exist in two completely separate compartments. Some ers may now point out that the proof will work only in the re[illegible]cted domain, and thereby claim to dispel the mystery. But their 'nditions' will still be extraneous to the proof-idea.[1] Better ption-barrers will glance quickly at the proof and gain, as I did [illegible] now, some inspiration for stating the conditions which determine a safe domain. The best exception-barrers do a careful analysis of the proof and, on this basis, give a very fine delineation of the prohibited area. In fact your method is, in this respect, a limiting case of the exception-barring method. . . .

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[1] '. . . Lhuilier's memoir consists of two *very distinct* parts. In the first the author offers an original proof of Euler's theorem. In the second his aim is to point out the exceptions to which this theorem is subjected.' (Gergonne's editorial comment on Lhuilier's paper in Lhuilier's [1812-13], p. 172, my italics.)

M. Zacharias in his [1914-31] gives an uncritical but faithful description of this compartmentalisation: 'In the 19th century, geometers, besides finding new proofs of the Euler theorem, were engaged in establishing the exceptions which it suffers under certain conditions. Such exceptions were stated, e.g. by Poinsot. S. Lhuilier and F. Ch. Hessel tried to classify the exceptions . . .' (p. 1052).

IOTA: . . . and it displays the fundamental dialectical unity of proof and refutations.

TEACHER: I hope that now all of you see that proofs, even though they may not *prove*, certainly do help to *improve* our conjecture.[1] *The exception-barrers improved it too, but improving was independent of proving. Our method improves by proving. This intrinsic unity between the 'logic of discovery' and the 'logic of justification' is the most important aspect of the method of lemma-incorporation.*

BETA: And of course I now understand your previous puzzling remarks about your not being perturbed by a conjecture being both 'proved' and refuted and about your willingness to 'prove' even a false conjecture.

KAPPA [*aside*]: But why call a 'proof' what in fact is an '*improof*'?

TEACHER: Mind you, few people will share this willingness. Most mathematicians, because of ingrained heuristical dogmas, are incapable of setting out simultaneously to prove *and* refute a conjecture. They would *either* prove it *or* refute it. Moreover, they are particularly incapable of improving conjectures by refuting them if the conjectures happen to be their own. *They want to improve their conjectures withou refutations; never by reducing falsehood but by the monotonous increase* [illegible] *truth; thus they purge the growth of knowledge from the horror of counte examples.* This is perhaps the background to the approach of the be sort of exceptionbarrers: they *start* by 'playing for safety' by devisi a proof for the 'safe' domain and *continue* by submitting it t [illegible] thorough critical investigation, testing whether they have made us [illegible] each of the imposed conditions. If not, they 'sharpen' or 'gene ise' the first modest version of their theorem, i.e. specify the lem on which the proof hinges, and incorporate them. For instance, after one or two counterexamples they may formulate the *provisiona exception-barring theorem*: 'All convex polyhedra are Euleria postponing non-convex instances for a *cura posterior*; next they devise Cauchy's proof and then, discovering that convexity was not really 'used' in the proof, they build up the lemma-incorporating theorem![2]

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[1] Hardy, Littlewood, Wilder and Pólya seem to have missed this point (see footnote 1, p. 125).

[2] This standard pattern is essentially the one described in the classic of Pólya and Szegö [1927], p. vii: 'One should scrutinise each proof to see if one has in fact made use of all the assumptions; one should try to get the same consequence

There is nothing heuristically unsound about this procedure which combines *provisional* exception-barring with successive proof-analysis and lemma-incorporation.

BETA: Of course this procedure does not abolish criticism, it only pushes it into the background: instead of directly criticising an over-statement, they criticise an under-statement.

TEACHER: I am delighted, Beta, that I convinced you. Rho and Delta, how do *you* feel about it?

RHO: I for one certainly think that the problem of ' ring-shaped faces ' is a pseudoproblem. It stems from a monstrous interpretation of what constitute the faces and edges of this soldering of two cubes into one—which you called a ' crested cube '.

TEACHER: Explain.

RHO: The ' crested cube ' is a polyhedron consisting of two cubes *soldered* to one another. Will you agree?

TEACHER: I don't mind.

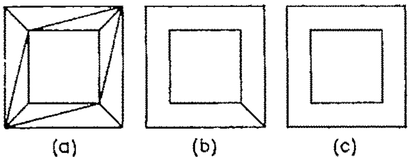

FIG. 14. Three versions of the ring-shaped face: (*a*) de Jonquières, (*b*) Matthiessen, (*c*) the 'untrained eye'.

RHO: Now you misinterpreted ' soldering '. ' Soldering ' consists of edges connecting the vertices of the bottom square of the small cube to the corresponding vertices of the top square of the large cube. So there is no ' ring-shaped face ' at all.

BETA: The ringshaped face is there! The dissecting edges you are talking about are not there!

RHO: They are just hidden from your untrained eyes.[1]

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from fewer assumptions . . . and one should not be satisfied until counterexamples show that one has arrived at the boundary of the possibilities.'

[1] This ' soldering ' of the two polyhedra by hidden edges is argued by de Jonquières ([1890*b*], pp. 171-172), who uses monsterbarring against cavities and tunnels but monster-adjustment against crested cubes and star-polyhedra. The first protagonist of using monster-adjustment in defence of the Euler theorem was Matthiessen [1863]. He uses monster-adjustment consistently: he succeeds in displaying hidden edges and faces to explain away everything that is non-Eulerian, including polyhedra with tunnels and cavities. While de Jonquières' soldering is a complete triangulation of the ring-shaped face, he solders with economy, by drawing only the minimal number of edges that split the face into simply-connected sub-faces (Fig. 14).

Matthiessen is remarkably confident about his method of turning revolutionary counterexamples into well-adjusted bourgeois Eulerian examples. He claims that

BETA: Do you expect us to take your argument seriously? What *I* see is superstition, but *your* ‘ hidden ’ edges are reality?

RHO: Look at this salt crystal. Would you say this it a cube?

BETA: Certainly.

RHO: A cube has 12 vertices, hasn't it?

BETA: Yes, it has.

RHO: But on this cube there are no edges at all. They are hidden. They appear only in your rational reconstruction.

BETA: I shall think about this. One thing is clear. The Teacher criticised my conceited view that my method leads to certainty, and also for forgetting about the proof. These criticisms apply just as much to your ‘ monster-adjustment ’ as to my ‘ exception-barring ’.

TEACHER: Delta, what about you? How would *you* exorcise the ring-shaped faces?

DELTA: I would not. You have converted me to your method. I only wonder why you don't make sure and also incorporate the neglected *third* lemma? I propose a fourth, and I hope, final formu[illegible] tion: ‘ All polyhedra are Eulerian, which are (*a*) simple, (*b*) have ea[illegible] face simply-connected, and (*c*) are such that the triangles in the pl[illegible] triangular network, resulting from stretching and triangulating, ca[illegible] so numbered that, in removing them in the right order, $V - E$[illegible] will not alter until we reach the last triangle.'[1] I wonder why you d[illegible] not propose this at once? If you really took your method seriously, you would have turned *all* the lemmas *immediately* into conditions. Why this ‘ piecemeal engineering ’?[2]

‘ any polyhedron can be analysed in such a way that it corroborates Euler's theorem . . .’. He enumerates the alleged exceptions noted by the superficial observer and then states: ‘ In each such case we can show that the polyhedron has hidden faces and edges, which, if counted, leave the theorem $V - E + F = 2$ untarnished even for these seemingly recalcitrant cases.’

The idea that, by drawing additional edges or faces, some non-Eulerian polyhedra can be transformed into Eulerian ones, stems however not from Matthiessen, but from Hessel. Hessel illustrates this point with three examples using nice figures ([1832], pp. 14-15). But he did not use this method to ‘ adjust ’ but, on the contrary, to ‘ elucidate the exceptions ’ by showing ‘ rather similar polyhedra for which Euler's law is valid ’.

[1] This last lemma is unnecessarily strong. It would be enough for the purpose of the proof to replace it by the lemma that ‘ for the plane triangular network resulting from stretching and triangulating $V - E + F = 1$ ’. Cauchy does not seem to have noticed the difference.

[2] The students are obviously quite knowledgeable about recent social philosophy. The term was coined by K. R. Popper ([1957], p. 67).

ALPHA: Tory turned into revolutionary! Your suggestion strikes me as rather Utopian. For there aren't just *three* lemmas. Why not add, with many others, conditions like '(4) if $1 + 1 = 2$', and '(5) if all triangles have three vertices and three edges', since we certainly use these lemmas? I propose that we turn only those lemmas into conditions for which a counterexample has been found.

GAMMA: This seems to me too accidental to be accepted as a methodological rule. Let us build in all those lemmas against which we can *expect* counterexamples, i.e. which are not obviously, indubitably true.

DELTA: Well, does our third lemma strike anyone as obvious? Let us turn it into a third condition.

GAMMA: What if the operations expressed by the lemmas of our proof are not all independent? If some of the operations can be performed, it may be that the rest must *necessarily* be able to be performed. I, for one, suspect that *if a polyhedron is simple then there always exists an order of deletion of triangles in the resulting flat network such that* $V - E + F$ *will not alter.* If there is, then incorporating the first lemma into the conjecture would exempt us from incorporating the third.

DELTA: You claim that the first condition implies the third. Can you prove this?

EPSILON: I can.[1]

ALPHA: The actual proof, however interesting, will not help us in solving our problem: how far should we go in improving our conjecture? I may admit that you have the proof you claim to have—but that will only decompose this third lemma into some new sub-lemmas. Should we now turn these into conditions? Where should we stop?

KAPPA: There is an infinite regress in proofs; therefore proofs do not prove. You should realise that proving is a game, to be played while you enjoy it and stopped when you get tired of it.

EPSILON: No, this is no game but a serious matter. The infinite regress can be halted by trivially true lemmas, which need not be turned into conditions.

GAMMA: This is just what I meant. We do not turn those lemmas into conditions which can be proved from trivially true principles.

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[1] Actually, such a proof was first proposed by H. Reichardt ([1941], p. 23). Also cf. B. L. van der Waerden [1951]. Hilbert and Cohn-Vossen were satisfied that the truth of Beta's assertion is 'easy to see' ([1932], English translation, p. 292).

Nor do we incorporate those lemmas which can be proved—possibly with the help of such trivially true principles—from previously specified lemmas.

ALPHA: Agreed. We can then stop improving our conjecture after we have turned the two non-trivial lemmas into conditions. In fact I do think that this method of improvement, by lemma-incorporation, is flawless. It seems to me that it not only improves but *perfects* the conjecture. And I learned something important from it: that it is wrong to assert that 'the aim of a "problem to prove" is to show conclusively that a certain clearly stated assertion is true, or else to show that it is false'.[1] The *real* aim of a 'problem to prove' should be to *improve*—in fact, perfect—the original, '*naive*' *conjecture* into a genuine '*theorem*'.

Our naive conjecture was 'All polyhedra are Eulerian'.

The monsterbarring method defends this naive conjecture by reinterpreting its terms in such a way that at the end we have a *monster-barring theorem*: 'All polyhedra are Eulerian'. But the identity of the linguistic expressions of the naive conjecture and the monster-barring theorem hides, behind surreptitious changes in the meaning of the terms, an essential improvement.

The exception-barring method introduced an element which i really extraneous to the argument: convexity. The *exception-barrir theorem* was: 'All convex polyhedra are Eulerian.'

The lemma-incorporating method relied on the argument—i.e. on the proof—and on nothing else. It virtually *summed up the proof in the lemma-incorporating theorem*: 'All simple polyhedra with simply-connected faces are Eulerian.'

This shows that (now I use the term 'proving' in the traditional sense) *one does not prove what one has set out to prove*. Therefore no proof should conclude with the words: '*Quod erat demonstrandum*.'[2]

BETA: Some people say that theorems precede proofs in the order of discovery: 'You have to guess a mathematical theorem before you prove it.' Others deny this, and claim that discovery proceeds by drawing conclusions from a specified set of premisses and noting the interesting ones—if you are lucky enough to find any. Or, to use a delightful metaphor of a friend of mine, some say that the heuristic 'zip fastener' in a deductive structure goes upwards from the bottom—the conclusion—to the top—the premisses,[1] others say that it goes downwards from the top to the bottom. What is your position?

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[1] Pólya ([1945], p. 142)

[2] This last sentence is from Alice Ambrose's interesting paper ([1959], p. 438).

ALPHA: That your metaphor is inapplicable to heuristic. Discovery does not go up or down, but follows a zig-zag path: prodded by counterexamples, it moves from the naive conjecture to the premisses and then turns back again to delete the naive conjecture and replace it by the theorem. Naive conjecture and counterexamples do not appear in the fully fledged deductive structure: the zig-zag of discovery cannot be discerned in the end-product.

TEACHER: Very good. But let us add a note of caution. The theorem does not *always* differ from the naive conjecture. We do not necessarily improve by proving. Proofs improve when the proof-idea discovers unexpected aspects of the naive conjecture which then appear in the theorem. But in *mature* theories this might not be the case. It is certainly the case in young, *growing* theories. This intertwining of discovery and justification, of improving and proving is primarily characteristic of the latter.

KAPPA [*aside*]: Mature theories can be rejuvenated. Discovery always supersedes justification.

SIGMA: This classification corresponds to mine! My first type of propositions was the mature type, the third the growing type. . . .

GAMMA [*interrupts him*]: The theorem is false! I found a counterexample to it.

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[1] Cf. Part I, footnote 2, p. 10. The metaphor of the 'zip fastener' was invented by R. B. Braithwaite; however, he talks only of 'logical' and 'epistemological' zip fasteners, but not of 'heuristic' ones ([1953], esp. p. 352).

(*To be continued*)

DISCUSSIONS

TWO EVOLUTIONARY THEORIES—A DISCUSSION

IN the pages of this *Journal* (1959, 9, 110-127, 185-193) under the title 'Two Evolutionary Theories', Marjorie Grene analysed and compared the mechanisms of evolutionary change advocated by Simpson in his *Major Features of Evolution* (1954) with those proposed by Schindewolf in his *Grundfragen der Paläontologie* (1950). This choice of works is an excellent one as these theories are directly opposed to one another in both their foundation and general philosophical significance. As a result of her study, Grene concluded that Schindewolf's theory is more adequate than Simpson's and more in agreement with her philosophical ideas—that a historical form of Kant's transcendental method must be applied in studies of evolutionary theory. We find ourselves, as students of 'macroevolution', in sharp disagreement with Grene's argument and conclusions. Lack of space prevents us from delving into all of the interesting problems arising from her study; we shall touch only upon the most important ones. The interested reader is referred to Schindewolf's and Simpson's works (and the literature cited by them) for scientific details and the evidence supporting our comments; we feel that an elaborate discussion of these data would be out of place in our note. We choose to substitute the term 'Synthetic theory of evolution' for Grene's 'Neodarwinism' as the former is the mor[illegible] accepted term for the school of evolution represented by Simpson; 'Neodarwinism' has been used for several different periods of evolutionary thinking and while mo[illegible] evolutionists are Darwinists, there is considerable disagreement on the exact meanin[illegible] of the term Neodarwinism.

Evolution and *phylogeny* are two of the most used as well as the most misused terms in evolutionary biology. Their exact definitions are rarely given as their exact use is frequently impossible to ascertain when reading an evolutionary treatise. Many students believe that evolution and phylogeny are absolute synonyms, when in fact they represent two sharply distinct ideas.[1] Unfortunately, a precise clarification of these terms and of the various aspects within evolutionary biology is lacking. Thus it is not surprising that Grene freely interchanged evolution and phylogeny, and claimed that evolution is a historical science. It is most unfortunate that Grene was misled into this false position by the recent evolutionary literature because the main factor preventing her from discovering the real difference between the rival theories is her confusion of evolution and phylogeny.

Any study of a historical subject, be it organic evolution, geology, western civilisation or whatever else, has three major aspects which are: (*a*) The description of the sequences of events and changes (=historical configurational sequences) which have occurred during time, these events are only described or listed without comment—the listing usually being in a chronological order; (*b*) The study of the mechanisms and processes, including causes, consequences, interrelationships with other influences, etc. (=immanent properties and processes), by which the subject matter changes; and (*c*) The overall explanation of the events and changes which have occurred using the known mechanisms of change. The first aspect concerns itself with a pure description of all the events that have happened and is a historical study. The second deals with how modifications can take place, how one set of events can cause a certain change or what one would expect if certain events had occurred. Although this branch deals with historical subjects, *it is not a historical study in itself.* The fact that the mechanisms and processes may operate over a period of time does not automatically make their study a historical one. Nor does their deduction from historical sequences make their study historical. The last branch, like the first, deals with the actual occurrences that had taken place and is also a historical study. Indeed, it is the ideal historical study for it includes all possible aspects, being a combination of the first two parts. We have listed all three aspects as separate subdivisions of historical study although other workers would prefer to place '*a*' and '*b*' as subheadings under '*c*' which they would regard as the complete historical study. To argue strongly for one arrangement or another would be quibbling. We shall follow the tripartite system because we feel that '*c*' is not a simple combination of '*a*' and '*b*' and because the tripartite system permits a better clarification of evolutionary biology which is our primary concern.

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[1] Consulting standard dictionaries is of no help for they follow the current practice of evolutionists and report the usual confusion between these terms. Indeed, analysis of the definitions given for evolution and for phylogeny in the second and third editions of Webster's New International Dictionary reveals that these terms are regarded as close synonyms of each other by the compilers of this work.

The entire field of evolutionary studies should be classified into these subdivisions nd names assigned to them. We would like to emphasise that this division is for urposes of clarification only and does not represent actual and separate branches inquiry. The overall field can be labelled as 'Evolutionary biology' and is vided into: (*a*) The description of all lineages of animals and plants from the origin of life to the present, the events being placed on a proper time scale—this may be called the study of phylogeny; (*b*) The study of the mechanisms of evolutionary modifications in organisms, be they large or small changes—this may be called the study of evolutionary principles, or the study of evolution; and (*c*) An explanation of the changes which have occurred in all lineages of animals and plants using the known evolutionary principles—an accepted term for this aspect is lacking, but it may be called evolutionary (or explanatory) phylogeny or the study of the evolution of animal and plant groups. Some workers would distinguish only between '*a*' and '*b*' which together would comprise '*c*' or evolutionary biology. Again we feel that this leads more easily to confusion. If one is concerned with the study of evolutionary theory or a principle of evolution, then one is dealing with the second subdivision of evolutionary biology *which is not a historical science.* Genetics and population biology fall into this category. When one speaks about the phylogeny of an animal group, or of a structure in an animal group, then the first branch is implied, while the evolution of an animal group or structure falls under the third heading. This distinction deserves further clarification as it is frequently confused. A pure description of animal lineages—which group follows which or which group gives rise to which—can be given without offering any explanation for the observed changes. This type of study is commonly done in paleontological work. It is phylogeny and such work should never be entitled 'The evolution of . . .' as is frequently done. When, however, the sequences of groups are given with explanations for the observed modifications, why new groups arose and why old ones died out, then one is looking for causes and must use the known principles of evolutionary change. Here one is really concerned with the evolution of animal groups and these studies can rightfully be called 'The evolution of . . .'. Both studies are concerned with phylogenies and are truly historical sciences. These remarks can be summed up by the following distinction: *While the study of phylogeny is a historical science, the study of evolution (= evolutionary theory) is clearly not historical.*

From these comments, it should be quite clear that for the most part, Grene was interested in theories of evolution, not in phylogeny; the title of her paper is 'Two Evolutionary Theories'. The pertinent differences between Simpson and Schindewolf are not in phylogeny—they both use the same knowledge of animal lineages; they differ in the evolutionary theories deduced from these facts. The facts used by Simpson and Schindewolf are historical ones, but the processes deduced from them are not. The processes are not historical, in the sense that they themselves do not change with time, but only their effects as seen in the configurations. Thus, Grene is wrong in saying that the study of evolutionary theory is a historical science (p. 187). Her methodological criteria under philosophical reflections, namely that the historical method is the necessary one in studies of evolutionary theories, must be rejected. It is still correct that the historical method is the basic one in all phylogenetical studies, both descriptive and explanatory.

Morphology and the synthetic theory are incompatible, one excludes the other. This is one of the strangest ideas advanced by Grene (pp. 115-116) and one that must be rejected as completely false. It may be true that Schindewolf's morphology i incompatible with the synthetic theory, but he represents a very small segment o present morphological thought. Grene shows confusion, for example, in he assumption that morphology must be typological. She consequently fails to see that it is typology, not morphology, that is inconsistent with the synthetic theory. The basic ideas used by most morphologists today are in complete agreement with the synthetic theory of evolution, if not based completely upon it. The synthetic theory is not founded upon the determination of ignoring structure, it is deeply concerned with examining and understanding structure as structure as well as its genetical and development bases. It is, thus our belief that by accepting Schindewolf's position as typically representative for all morphologists, Grene was badly deceived about the true relationship between morphology and the synthetic theory and was unable to understand Simpson's inquiries into the genetical bases of structure and of macroevolutionary changes.

Much of the difference between the two rival theories could be eliminated if the different sets of terms used were translated which Grene claims is possible and suggests be done (p. 117). She notes that Schindewolf and Simpson would object strongly to this suggestion, to which we would add, quite rightly so because they know the connotations of the terms as well as their meanings. Grene believes that many terms such as 'orthogenesis' could be interchanged for 'essentially rectilinear evolution' or 'general improvement' for 'general adaptation' without loss of meaning. This cannot be done because the implications of these terms go further than their definitions. Schindewolf, for example, implies much more than mere description when he uses such terms as 'type', 'general improvement', 'bauplan' and 'orthogenesis'; these additional implications are objectionable to students of the synthetic theory. And Schindewolf would object to the connotations associated with the terms used in synthetic theory. Both know that there is more to their terms than the explicit meaning. Perhaps these connotations should be clearly stated by the author when he defines terms; however, this is not done, and really cannot be done without making the scientific literature completely boring. A knowledge of the hidden meanings is expected of those reading the scientific literature. Because these hidden meanings are among the most subtle parts in the evolutionary literature, extensive reading is required before they can be easily recognised and separated. Simpson's and Schindewolf's terms are strikingly different in their connotations, and we disagree with Grene that the verbal differences between their theories would disappear if each term was interchanged for the 'corresponding' one. The verbal differences are not the result of a simple use of different words, but are the result of clearly separable ideas which are expressed by the use of different terms—words which cannot be translated.

Analysis of ontogenetic change and correlation between ontogenetic and phylogenetic change form the basis for much of Schindewolf's theories. Grene (pp. 119, 122-123) accepts these ideas and summarises them into the concept that phylogenetic relationships can be shown with 'reliance on analogies drawn from embryology', to which we object strongly. Phylogeny and ontogeny are both time oriented changes, but they are basically different processes. Ontogeny is the change in an individual from its formation until its death, while phylogeny is the change of organisms through a succession of generations. For sexually reproducing species, phylogeny must be thought of as a population phenomenon. Phylogeny may be considered to be a succession of ontogenies, and alteration in ontogeny certainly form the basis for phylogenetic changes. But it is not possible to ascertain what has happened during past phylogeny by examining the sequence of events in an ontogeny, nor is it possible to claim that ontogenetic changes can tell us anything about phylogenetic changes—that these processes are subdivisions of a major overall process and hence that modification in one (ontogeny) means automatic and equivalent modification in the other (phylogeny). We feel that the burden of proof rests largely with those who still claim such a relationship.

The importance placed by Schindewolf on early ontogenetic change is based on the idea that the earlier a change occurs in ontogeny, the greater will be the resulting modification in the adult structure—and hence, for Schindewolf, the greater will be the taxonomic change in the type. This argument is grounded upon the same basic thinking that has led to the formulation of the 'Biogenetical law' (Ontogeny recapitulates phylogeny) by Haeckel, in spite of the fact that Schindewolf argues strongly against this law. Both concepts are built on the belief that features appearing earliest in the phylogeny of a group appear first in the ontogeny, followed by the features emerging later and later in the phylogeny. That is, the features characteristic of the phylum appear first in embryological development, followed by those of the class, order, family, genus and last of all the features of the species appear. What appears first in ontogeny must also have appeared first in phylogeny. The arguments put forth by de Beer and others against the Biogenetic Law can be applied equally well against the ideas of Schindewolf.

There are other areas in which Grene displays a lack of understanding for the facts of modern biology by adhering too closely to Schindewolf's book, but space does not permit us to delve into these points. A major problem rests with Schindewolf's and Grene's failure to realise that evolutionary change in sexually reproducing animals *must* occur within an interbreeding population. Individuals are always members of a population and cannot singly form the basis for any evolutionary changes. Evolution is the modification of a group of interbreeding organisms, not of independent individuals. Here lies the main error in Schindewolf's theory. Grene has not appreciated this failure and hence falls into the same trap that caught Schindewolf. The concept of Typostrophism advanced by Schindewolf illustrates his strict thinking of evolutionary change in terms of modification in individuals with a complete disregard for populations.

The basis for the theory of typostrophism lies in Schindewolf's concept of 'type' and 'bauplan'. Type is defined by Schindewolf (p. 240) as the characteristic features of a taxonomic group. Bauplan is almost synonymous with type, perhaps it is best defined as the arrangement of the characteristic features in the animal. This definition is purely descriptive and is not different from the concept of the characteristics of a group or its morphological definition used in the synthetic theory. But, Schindewolf puts more into his type concept which is not mentioned in his definitions. These additional connotations are never defined or clarified in spite of their forming the most significant part of the type concept. Let us first call attention to the fact that Schindewolf defines the type on the basis of the taxonomic group—that is, the type is the sum of the diagnostic features of a taxonomic group. Most workers agree that the limits of taxonomic groups are established rather artificially. This is not to say that the groups are not real, but that their exact limits and which forms are included in a certain group, as well as the taxonomic ranking of this group, depend[illegible] upon the opinion of the individual worker. Hence what one worker would call [illegible] family, another may regard as a genus, or what one worker would call a genus, another may divide into two genera. Thus according to Schindewolf, the type or the bauplan would vary according to the opinion of the worker defining the taxonomic group. And since taxonomic groups are erected according to the known variation of morphological characters, care must be taken when using the limits of these groups to establish the possible limits of variation of types—one can very easily fall into the trap of circular reasoning. Schindewolf goes past his type definition when discussing the type border or limit (Typengrenze, pp. 249, 305-306), and type-defining characters (typenbestimmenden Merkmale, p. 249), or in using specific characters, generic characters, etc. (p. 268), yet never offering one word of definition for these additional parts of his type concept. In the absence of such clarifications we shall try to deduce what he means by these terms.

The type border is not simply the observed limit of variation in a taxonomic group, but is an absolute, hard and fast boundary—the real limit of the type. When the boundary of the type is passed, which is accomplished only by special evolutionary steps, a new type is reached. The type limit cannot be stretched or enlarged, it is fixed. The main or defining character of the type is not simply a useful feature for recognising that particular taxonomic group—a key character—but it is the basic feature of the type; the feature which appeared when the new type evolved by the single step jump from the old type—the character by which the new type is recognised. Species specific characters, generic characters and so forth are obvious—they are the main features of the species, genus, and so forth. According to Schindewolf, these are characters of increasing importance which one can recognise, *a priori*, as being the features of a new species, genus and so on, but nowhere does he give any hint how one recognises these characters and why they indicate taxonomic groups of different ranks. Most workers agree that there is no such thing as a ' species character ' or ' generic character ', but that the group is established first and that the group gives the character, not the character giving the group.

According to Schindewolf's concept of the type, animals intermediate between types cannot exist, they must belong to one type or another. And the change from one type to another must be a single step jump, otherwise there would be intermediate forms and these cannot exist. In this initial step—the jump from one type to another—the main feature of the new type must appear, otherwise it would not be possible to recognise the new type. Hence the main characters of a new order appear in this first step and in the first individual of the new type if the change happened to be one of ordinal value. Herein lies the problem. An individual may cross the type boundary because of some early change in its ontogeny resulting from a large mutation. Even ignoring the need for explaining the large mutation, and how this radical change in one or a few characters can function harmoniously with the other features of the organism and in the possible environment of the new animal, one could ask with which individuals would this single member of the new type interbreed if it is a sexually reproducing organism? The chances of a second individual of the opposite sex making the ' type jump ' at the same place and same time is infinitesimally small. Thus, in the large majority of cases, the single individual of the new type can only interbreed with members of the old type, leaving the question of what the offspring would look like and whether the mutation leading to the ' type jump ' would show Mendelian segregation when the new type bred with members of the old type. We also wonder how the appearance of a new type would allow the animal to be momentarily withdrawn from the confining influence of selection (Grene, p. 123), selection being always the result of the interaction between the organism and the environment. No selection means no environment which violates the basic concept that organisms must always be considered in relationship to their environment at all stages of their life cycle. These shortcomings of Schindewolf's typostrophism emphasise his neglect to regard animals as individuals grouped into interbreeding populations living in an environment which are basic prerequisites for any theory of evolution.

Schindewolf's evolutionary theories present little problem for students of evolution; they have the background to judge them and accept what they believe to be correct. They do present a far greater danger to students of the philosophy of science. Grene (pp. 191-193) concludes that the ideas of Schindewolf are simpler, include an ordering principle and a concept of novelty or originality which are more in agreement with her philosophical beliefs. Such a conclusion is not really surprising, but it may also be meaningless. Schindewolf's evolutionary theories are derived directly from the earlier ideas of the German 'Naturphilosophie' and hence have a very definite philosophical basis. Naturphilosophie has been strongly influenced by, if not completely derived from Kant's transcendentalism. Therefore to argue, as does Grene, that Schindewolf's evolutionary theories support her philosophical concepts would be circular reasoning if both theories stem from Kantianism. If a philosopher wishes to use a set of scientific ideas as independent support for certain philosophical concepts, he must first establish beyond any doubt that these scientific ideas are not based or strongly influenced by these philosophies. Grene has not ascertained, or even inquired into the historical background of Schindewolf's evolutionary theory when making her claim that it offers better support for her philosophical beliefs. Even if her analysis of the rival evolutionary theories was correct, her argument for favouring Schindewolf appears to be completely circular and hence is incapable of solving the major problem set forth in the beginning of her study.

Walter J. Bock
Gerd von Wahlert

On Evolutionary Theories

M. Grene[1] has presented a comparison between the evolutionary theories outlined by Schindewolf[2] and Simpson.[3] She concludes that there is little biological difference between them and that Schindewolf's is preferable philosophically. I wish to take issue with both these points, and add certain general comments.

Schindewolf's approach is to a considerable extent that of an idealist in the tradition of Plato, Goethe, and Owen, while that of Simpson is more purely materialistic, an approach beautifully analysed in its relation to evolution by Beckner.[4] It is at this point that the first fundamental biological difference appears between the two theories.

According to Schindewolf, a higher taxon (a classified group of organisms actually exists when its first species appears. According to Simpson, a higher taxo does not exist (except retrospectively) until it has diverged or diversified sufficient for it to be classified as separate from its immediately ancestral taxon of the same ran The former view implies that higher taxa exist objectively, with sharp boundaries; the latter view holds that higher taxa have no exact natural boundaries in time and that more than one valid classification is often possible on the basis of the same biological information even if this information is complete.[5]

Secondly, although related to the distinction just made, the mechanisms advocated for the origins of higher (supraspecific) taxa, and species as well, are utterly different. Schindewolf proposes that each taxon of whatever rank originates full-blown (*mit einem Schlage*) from a preceding one by a single '*typostrophische*' mutation of a large effect (*Grossmutation*), and that the resulting individual or individuals ('hopeful monsters' in the phrase of Goldschmidt)[6] are at their appearance immediately reproductively isolated from the parental population. Simpson, on the other hand, believes that except for the relatively uncommon situations of polyploidy and neoteny (the former a very different mechanism from Schindewolf's, the latter only partly compatible with Schindewolf's and leading rapidly to a regression into the one-cell stage if no further process is operative), species and *ipso facto* higher taxa always originate by gradual transformation of one or more populations of one (or at most a very few) ancestral species, reproductive isolation gradually accumulating. Each writer explicitly denounces the view of the other on this point, which I believe is the greatest point of mechanistic difference of any two responsibly advocated evolutionary theories in the last thirty or more years.

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[1] This *Journal*, 1958, 9, 110 and 185

[2] *Grundfragen der Paläontologie*, Stuttgart, 1950

[3] *The Major Features of Evolution*, New York, 1953

[4] *The Biological Way of Thought*, New York, 1959

[5] See Simpson, *Principles of Animal Taxonomy*, New York, 1961. All references to this book are more elaborate statements of positions taken by Simpson in 1953 or earlier.

[6] *The Material Basis of Evolution*, New Haven, 1940

It is of course possible that the differences discussed by Grene were psychologically involved in the development of these different theories, but they are not relevant to a consideration of the merits of the theories themselves.

There are further purely biological differences between the theories of Simpson and Schindewolf (e.g. in the regularity and meaning of ' racial senescence '), but these are of less importance than the two described above and may be ignored.

The morphology of Schindewolf is based on the scholastic premise that each taxon has a set of basic structures (*Bauplan*, *Typus*, *Organisationsgefüge*, etc.) on which are superposed the observed variations (if any) between the included taxa and between the member individuals. Variation therefore partly conceals the meaningful morphology and is a hindrance to investigation. Simpson accepts variation as being important both in itself and in its relation to the evolutionary process, and considers a description of variation to be an important part of any description of the true morphology of a taxon. Variation ' belongs to the very nature of taxa'.[1] This philosophical difference will be discussed below.

A philosophical journal is not the place to consider in detail the biological correctness of the theories (the one presented by Simpson is, in its essentials, adhered to by the overwhelming majority of biologists), although a few points of possible philosophical interest may be noted. In the first place, there are intermediates known between many taxa, as Schindewolf verbally recognises. They are nevertheless usually less common than the more central representatives, as he emphasises and as Simpson attempts, in my opinion successfully, to explain. With reference to the most famous of these, *Archaeopteryx*, which is precisely intermediate in structure between birds and reptiles,[2] Schindewolf states that it is a true bird. It does, of course, have many traits in common with birds, including the usual ' key ' character of feathers, and therefore has what may be defined as the *Bauplan* of birds. But this definition of the avian *Bauplan* can be made only after a study of *Archaeopteryx*, since a *Bauplan* inferred from later birds would not apply in many important respects to *Archaeopteryx* and so *Archaeopteryx* could not then be stated to be a true bird. The *Bauplan* of either reptiles or birds could be with equal validity extended so as to apply to *Archaeopteryx*. Schindewolf chose to extend the avian *Bauplan* rather than the reptilian one presumably because *Archaeopteryx* is definitely more birdlike than (?other) reptiles and provides a convenient beginning for the class Aves (it would be an inconvenient side-branch of the class Reptilia). This is why it is perhaps always placed with the birds. Incidentally, the transition between reptiles and mammals cannot now be treated in the Schindewolfian fashion except in a baldly arbitrary manner. Hundreds of forms intermediate between typical (more precisely, amphibian-like) reptiles and typical mammals are now known and form a complex pattern of lineages with no gaps even nearly as large as those on both sides of *Archaeopteryx.*[1] In fact the boundary between these two classes is so arbitrary that no less than four very different positions for it to have recently been advocated.

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[1] *Principles of Animal Taxonomy*, p. 50

[2] The most recent comprehensive study is that of G. R. de Beer, *Archaeopteryx lithographica*, London, 1954.

As to the second biological difference, there is quite simply no known mechanism that could possibly be of any general occurrence, for the process postulated by Schindewolf, while the process invoked by Simpson remains hypothetical in no essential point. If Simpson's theory is to be supported by this difference, the adoption of a criterion of simplicity[2] seems to be necessary. Grene would nevertheless perhaps choose to accept Schindewolf's theory despite its lack of a known mechanism, since she proposes in effect the contradiction of Occam's razor, namely that a theory should not be rejected unless there is evidence against it (' aspects of reality should not be *subtracted* beyond what is honest').[3] Particularly in systems as complex as most of those with which biologists work, a principle such as that espoused by Grene must always be borne in mind. However, I believe the valid portion of Grene's principle is more adequately rephrased as follows: Some hypothesis other than the simplest one may actually be true. In this form it is clearly consistent with accepting the simplest hypothesis as a basis for further action. The less simple hypotheses then remain in the twilight limbo of the potential or the conceivable until or unless they are disapproved or in turn become the simplest hypotheses consistent with the available evidence.

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[1] This has been true for thirty years or more (R. Broom, *The Mammal-like Reptiles of South Africa and the Origin of Mammals*, London, 1932), but recent discoveries are even more conclusive. See, e.g. papers by Olson, Simpson, Van Valen, and Reed in *Evolution*, 1959 and 1960, and references cited therein.

[2] I tend to accept Popper's identification (*The Logic of Scientific Discovery*) of the ease of falsifiability as the most important element of the simplicity of a hypothesis (but see below). I disagree with him (p. 142, 1959 edition), although possibly only verbally, on one point. Surely if we adopt a hypothesis, even tentatively, we believe it has the highest prior probability of being correct, of all possible hypotheses fitting the data available. It is relatively improbable that it fits the data as a result of chance (Popper's ' logical improbability '), so it is therefore relatively probable that the hypothesis is correct. It is in any event normally as a basis for further action or investigation tha a hypothesis is accepted, whether or not this further action is consciously planned to be [illegible] test of the hypothesis. I therefore believe that, logically speaking, simplicity is an ethic rather than an epistemological criterion, although its subject matter is epistemologic perhaps all epistemological criteria are of this character.

Suppose, however, that we take five marbles at random from a large box filled with marbles of various colours. Three of these five marbles are red. Many hypotheses of the proportion of red marbles in the box are now permitted by the available data; one of the more easily falsifiable ones is that at least four-fifths are white. But this is nevertheless a less probable hypothesis than the one that between five and seven-tenths are red, despite the fact that fewer marbles must on the average be drawn to prove it false at any desired level of confidence. In this case the simpler hypothesis by the criterion of paucity of assumptions coincides with (or is determined by) the maximum likelihood estimate, while being less easily falsifiable than various other hypotheses. I am not, however, prepared to assert that the criterion of paucity of assumptions (or of parameters, but not of information) is universally applicable in preference to the criterion of ease of falsifiability, although this may well be true. Uniformity with previous experience (we should act as we or others have acted before if new data permit us to do so) is an aspect of what is here called paucity of assumptions.

[3] Grene, op. cit. p. 193. Grene has also made the following comment (in a letter): 'I am not simply saying : some hypothesis other than the simplest may be true. I am saying : an hypothesis should not be accepted which *pretends* to do without concepts that it does, in fact, surreptitiously use. This is what Polyani calls *pseudo-substitution* ; Simpson's treatment of " type " is a case in point.' The rejection by Simpson of ' type ' in the sense of Schindewolf does not mean rejection of various other concepts also sometimes named by this word ; it appears to me that Simpson is consistent in his rejection of Schindewolf's types. As discussed in the text below, Grene's definition of ' type'

Beckner[1] has given an excellent account of the relations between levels of explanation; he has also shown that the supposed problem of emergence emphasised by Grene does not now exist. Neo-Darwinians use several levels of explanation[2] as a matter of course (molecular movement in gene replication is obviously on a different level from the migration of populations), despite Grene's assertion.[3] Structure is not an epiphenomenon for neo-Darwinians, arising out of gene action and then unimportant, but provides the basis for the highly complex interaction of genetic systems with the environment. A major theme of Grene's paper, that neo-Darwinism ignores the structure, patterns, and integration of organisms, is simply false.[4] This is so obvious to a biologist as not to need explicit mention in a book intended, on the highest level, for biologists rather than philosophers. That probably most neo-Darwinians believe (because of a criterion of simplicity) all levels to be potentially explicable physico-chemically is a related (cf. Beckner) but different matter.

A further point of difference, partly biological and partly philosophical, concerns the process by which evolution is directed. Both Simpson and Schindewolf explicitly reject finalism, the theory that evolution is controlled by a pervasive force cting toward a goal. According to Schindewolf, the influence orienting evolution in its important aspects internal to the organism (i.e. mutation), and not a product f natural selection, which Simpson believes to be the most important factor. Natural lection, simply by definition, usually improves the ability of the species to survive

not that to which Simpson objects. In general, the remedy for genuine cases of pseudo-substitution would seem to be the introduction of the concept elided out rather than an even provisional rejection of the hypothesis because of the pseudo-substitution.

I believe the most important difference between Grene and myself is in the relative importance we attach to a criterion of simplicity. To me such a criterion is of overwhelming importance not only in science and other intellectual activities but in everyday life, although its use is usually so automatic that we are usually unconscious of employing it. I do not, for example, believe that a murderer is behind me as I write this, although I have not looked behind me to falsify the hypothesis. We cannot and do not act on the basis of every conceivable hypothesis. Similarly, a theory more or less adequately tested on one level (e.g. the level of microevolution) must be assumed to apply to other levels also unless definite contra-indications can be shown to exist.

[1] Op. cit.

[2] Cf. the articles by Grobstein, Simpson, Mommaerts, and Waterman in *American Scientist*, 1962, 50

[3] Op. cit. p. 190

[4] Of literally hundreds of counter references, four may be cited: B. Kurtén, *Acta Zoologica Fennica*, 1953, 76; E. C. Olson and R. L. Miller, *Morphological Integration*, Chicago, 1958; L. Van Valen, *Evolution*, 1962, 16, 272; G. G. Simpson, *Proc. Amer. Philos. Soc.*, 1959, 103, 286. Various earlier papers are cited in these.

in the environments in which it exists,[1] and the evidence (both direct and indirect) for the importance of natural selection is overwhelming.[2] The survival of instantaneously-produced new taxa, in Schindewolf's theory, is due to their being in some way at least often well adapted when they appear. Schindewolf explicitly rejects selection as an important factor, but there is no other known mechanism for the production of well-adapted taxa. The large majority of known small mutations are deleterious, in general the greater the effect of a mutation the greater the chance of its being deleterious, and in general a mutation is less deleterious in single dose (the condition in which it must be when it appears) than in double dose, in which condition it must be to maintain a true-breeding population. To say, as does Grene,[3] that the new taxon 'is withdrawn momentarily from the confining influence of selection, and proliferates, out of sheer exuberance; in all sorts of directions', is to beg the question. The difficulty is precisely that there is no evidence that this happens, in addition to any known way for it to do so (in fact it is impossible for it to happen, by the very nature of natural selection[4]). A criterion of simplicity must again be invoked here.

The remaining point of importance deals with typology, with the nature of a *Bauplan*. In his reaction against idealistic morphology, Simpson states[5] that 'the strictly typological approach should have no part in modern taxonomy', aside from the simple use of characters-in-common.[6] This is quite different from saying that types in Grene's sense[7] of 'general patterns of structure or function' are of no valid use, and Grene points out that Simpson uses such 'types' as a matter of necessity. It is difficult to see with what she disagrees here other than his refusal to use the ambiguous word 'types' for such patterns. If Simpson's emphasis on the historical continuity of life and of organismic patterns is antithetical to a neat partitioning o the universe, then so much the worse for a neat partitioning. I have, howeve elsewhere[8] indicated that even set theory is amenable to the adequate descripti of the gradations inherent in a continuum. In addition, the presence of a phylogenet continuum in the past is not inconsistent with the existence of discrete and wide ga at present, which are most obvious when only a small geographical region is considered. I would agree (I am not sure that Simpson would) that 'types' (or sets, or partitions) are often useful in the description of discontinuities and even of continua, but not, as Schindewolf believes, that they are universal and real in the process of evolution.

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[1] It does not necessarily do so in such relatively minor variants as gametic and sexual selection.

[2] J. B. S. Haldane, in *Darwin's Biological Work* (P. R. Bell *et al.*), Cambridge, 1959 ; G. G. Simpson, *Major Features* ; P. M. Sheppard, *Natural Selection and Heredity*, New York, 1958

[3] Op. cit. p. 123

[4] A philosopher who questions this statement would do well to read the books cited in footnote [2] on p. 150, and also (but not instead) Beckner's excellent analysis (op. cit.). Natural selection is, and was to Darwin (but not to various other people, including perhaps most contemporary philosophers), quite simply the production of different average numbers of offspring, for whatever reason other than chance, by individuals with different genetic constitutions. The individuals that produce the most offspring are by this very fact the best able to maintain the species in its current environment.

[5] *Principles of Animal Taxonomy*, p. 50

[6] Simpson does not seriously object to the use of typological terminology (as opposed to the masking of typological concepts in neo-Darwinian terminology), as Grene claims. For example, he praises (*Quart. Rev. Biol.*, 1958, 33, 148) a work by Heberer and Zimmerman in which the terms '*Typus*' and '*Typogenese*' are prominently used.

[7] Op. cit. p. 192

[8] In a volume now in press.

The concept of a *Bauplan* in the sense of Woodger,[1] a set of characters-in-common, is used by every taxonomist and morphologist. The idealist adds to this two things. The first is a broader concept of *Bauplan*,[2] namely a pattern from which the observed patterns in the set of organisms under consideration can be derived. (The fact that there are always or almost always many such patterns [3] does not deter the idealistic morphologist from choosing one of them, by partly-defined rules, and maintaining it as the real and important pattern from which the rest are deviations.)

The second addition is the proposition that the *Baupläne* are coextensive with natural taxa, i.e. that *Baupläne* and taxa have a one-to-one correspondence. This proposition is vulnerable from both sides. Not all taxa are definable in terms of *Baupläne*,[4] and many *Baupläne* include some but not all members of two different taxa. With regard to the example mentioned earlier, the presence of separate first and second metacarpal bones is part of a reptilian *Bauplan*, but they are also separate in *Archaeopteryx*; they are fused to each other and the carpals in all other birds. Such examples are ubiquitous and serve, in conjunction with other criteria, to indicate the transitional nature of many taxa.

If taxa are discrete in time, then the organic world can in theory be objectively partitioned. If however taxa grade into each other, partitioning will be to a greater or less extent arbitrary no matter how much information is available. If *Baupläne* are unambiguously determinable, then they and not the deviations from them are of primary importance. But if *Baupläne* are partly or entirely subjective, then we are forced to look closely at individuals, at the patterns of individual and group variation, for these are not now distracting nuisances but have become the keys to understanding.

Four additional points raised by Grene deserve brief comment, particularly because the viewpoint behind them is rather widespread among philosophers.

Non-adaptive and inadaptive characters are supposed to be unexplained by neo-Darwinism. I have elsewhere [5] reviewed the rather large number of neo-Darwinian mechanisms for the production of such characters, with probably an underemphasis on the important mechanism of the necessary interrelations of different characters in development, or developmental requirements.

A statement of Simpson's quoted unfavourably by Grene, 'It is certain that if we can see any advantage whatever in a small variation (and sometimes even if we cannot), selection sees more', is based, as Simpson says,[6] on mathematical demonstration of the efficacy of such slight selective advantages *under biologically usual*

[1] In *Essays on Growth and Form* (W. E. Le Gros Clark and P. B. Medawar, eds.), Oxford, 1945, p. 104. He is, to my knowledge, the only person to use '*Bauplan*' with this restricted meaning.

[2] R. Zangerl, *Evolution*, 1948, **2**, 351. It should be noted that a few idealistic morphologists look favourably on neo-Darwinism.

[3] Cf. L. Van Valen, *Jour. Paleont.*, 1963, **37**, 257

[4] See Simpson, *Principles of Animal Taxonomy* and *Major Features*; also Van Valen, *Bull. Amer. Museum Nat. Hist.* (in press).

[5] *American Naturalist*, 1960, **94**, 305

[6] *Major Features*, p. 118

conditions. It does not seem to be sound practice to criticise a conclusion, possible on its face and without contrary factual evidence available, while ignoring or accepting the assumptions and reasoning behind it. I believe Simpson's statement should be qualified, but only slightly, and not in such a way as to alter the argument for the power of selection.

The view that neo-Darwinians reject the possibility of any kind of apparently abrupt transition as a result of early ontogenetic change is incorrect,[1] although this phenomenon is regarded as rare and Schindewolf's mechanism is rejected.

The peculiar notion[2] that selection is not a mechanism of evolution is easily dealt with. Evolution is a result of systematic changes in gene frequencies, by the neo-Darwinian theory, and selection is a major mechanism for the production of changes in gene frequencies, systematic or otherwise. In a system consisting of self-reproducing genes or gene analogs and any accessory material (e.g. life, as regarded from the viewpoint of the purpose of existence of any individual species), natural selection can by itself produce a change in gene frequencies. An automatic agency for the production of a physical change is part of my concept of a mechanism.

The most important philosophical difference between Simpson and Schindewolf, according to Grene, is the more abstract, 'reductive' approach of Simpson as against the more specimen-oriented, one-level approach of Schindewolf. However, both approaches are possible (and used by different individuals) under neo-Darwinism. Mutation or non-additive gene action is necessary to explain novelty (emergence) in evolution, but if mutations can be accumulated a single mutation clearly does not have to involve a complete structural reorganisation.

I would say that the most important philosophical difference between Simpson and Schindewolf, one also recognised in part by Grene, is that Schindewolf appear[illegible] to start from the differences and gaps in the organic world, regarding intragroup variation as relatively unimportant, while Simpson appears to start from the observed and inferred continuities, regarding intragroup variation as of great importance and essential to any evolution. This difference is related to their attitudes towards idealistic morphology. But of whatever philosophical importance this difference may or may not be, its real basis is a biologically decidable difference between two coherent theories, and therefore the choice between the alternatives should be made on biological rather than philosophical grounds.

LEIGH VAN VALEN

TWO EVOLUTIONARY THEORIES: A REPLY

THE disagreements and misunderstandings between my critics and myself seem to me, five years after I wrote my paper (this *Journal*, 1958, 9, 110–127, 185–193), to stem primarily from the want of adequate philosophical concepts at the foundation of biology (in general, not only in evolutionary theory)—concepts which we *all* lack, since, if they exist at all, they are still in process of formation. I would refer the authors in this connection especially to such writings of A. Portman as *Neue Wege der Biologie* (Piper, 1961), and *Zoologie und das neue Bild des Menschen* (Rowohlt, 1956) as well as to Michael Polanyi's Terry lectures of this year (and to *Personal Knowledge*, 1958, Pt. IV).

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[1] Simpson, *Major Features*, p. 97 ; G. R. de Beer, *Embryos and Ancestors*, Oxford, 1958

[2] M. Grene, this *Journal*, 1958, 9, 122, footnote

Meantime, however, I should mention some points on which I believe that their criticisms can be answered within the available framework.

1. '*Evolution*' and '*phylogeny*'. (*a*) This is not so usual a distinction as the authors claim. True, their distinction between 'events' and 'processes' (historical *v.* experimental) is made, e.g. by Simpson in 'The Study of Evolution and Its Record' (Roe and Simpson, *Behavior and Evolution*, Yale, 1958), but he goes on to say (p. 13) that they cannot be kept apart—and he applies 'evolution' *throughout* to the historical type of study.

(*b*) If the experimental and historical are really kept separate, i.e. if the results of detailed studies in the short run are *not* extrapolated to phylogeny, then of course there can be no objection to them. That does not seem to be the gist of *Major Features*.

2. *Morphology and phylogeny*. I did not argue that 'morphology and neo-Darwinism are incompatible', but that *traditional* morphology, from which Schindewolf starts, must be, by Simpson, radically revised. I can only refer readers to Simpson's works, including his recent *Principles of Animal Taxonomy*, for confirmation of this point.

3. *Ontogeny and phylogeny*. (*a*) A reference to ontogenesis in connection with evolution (phylogeny?) does *not* entail the reassertion of Haeckel's discarded principle. The reaffirmation of von Baer's conception in this context, along with the theories of Garstang and others *re* paedomorphosis and neotony are essential to Schindewolf's position, as they are to that of Dalcq and others. In other words, Garstang, etc., does not entail synthetic theory any more than some other theory drawing from embryology entails Haeckel. Quite the contrary: modern embryology *refutes* Haeckel and *suggests* possible saltative aspects in phylogeny along Garstang lines.

(*b*) Of course nobody asserts that 'individuals' evolve, only populations. But populations are made up of individuals in whom the mutations and recombinations responsible for evolutionary (phylogenetic?) change occur. Phylogeny is certainly a series of ontogeneses, and development certainly has a bearing on phylogeny. See the writings of Waddington, Schmalhausen, Dalcq, Lillie and many others.

4. *Philosophical method*. (*a*) The fact that I refer to Kantian (i.e. transcendental) *method* (see, e.g. Strawson, *Individuals*, p. 40) does not in the least indicate that I hold any Kantian *beliefs*—nor does Kant have anything to do with 'Naturphilosophie'.

(*b*) I could scarcely have been biased in favour of Schindewolf before beginning my analysis as I knew nothing of him before starting to study the literature.

(*c*) I am of course aware of the possible affinities of Schindewolf with *Naturphilosophie;* that was not relevant to my argument; for even if *Naturphilosophie* was on the whole a bad influence that does not imply that any position influenced by it is necessarily false. Moreover, I am also aware of the philosophical inadequacy of the American concept of adaptation with which Simpson's school (see, e.g. Pitendrigh in *Behavior and Evolution*) is infected, but I do not hold *that* against Simpson—I am comparing two *arguments*, not two sources of 'conditioning'.

(*d*) Nor did I argue that Schindewolf's theory gave an adequate explanation, but only that it *is epistemologically consistent with one*, which neo-Darwinism is not—(see Polanyi on ' pseudo-substitution ' in *Personal Knowledge*).

5. May I take this opportunity to mention that Part Two of my paper should have opened ' These are two ways . . . '; not ' There are . . . '. I do not by any means hold that there are only two possibilities—nor that Schindewolf's ' type theory ' is adequately explanatory. Only that it is not self-contradictory and so is at least a possible starting-point for *asking* philosophical *questions*—not for giving philosophical answers, as my critics suggest I mean to do.

MARJORIE GRENE

PROFESSOR MACKAY ON MACHINES

IN a recent article[1] in this *Journal*, D. M. MacKay concludes that behavioural signs of consciousness cannot ' relieve our ontological doubt ' about the possibility of consciousness in other people, animals, or artificial mechanisms. He suggests several criteria whose satisfaction might, however, incline us to grant that some machines can (in principle) think, and have other characteristics of personality. While I agree with Professor MacKay that a ' thinking machine ' is not a contradiction in terms, I find portions of his argument obscure or else irrelevant. The necessary ' ontological doubt ' seems to me to have been introduced erroneously.

Professor MacKay's argument has three major points. He argues that: (1) A necessary condition for ' pretensions to human powers ' is an underspecified situation, i.e. a physical situation to which more than one description can be justifiably applied; (2) a condition both necessary and sufficient for us to recognize some object as having personality is that ' we, the arbiters, must be rationally able and willing to adopt the appropriate standpoint of interactive relationship, from which the situation acquires some of our own necessary underspecifiableness-to-ourselves ' (p. 96); (3) a necessary condition for the fulfilment of (2) is the fulfilment of (1).

The notion of ' underspecifiability ' admits of two interpretations, each of which is required to support some portion of his argument. (*a*) First, underspecifiability is a function of the completeness of the description we do (rather than can) assign to an object. Initially, it is said:

> Only if we *under-specify* the situation by using generic terms that allow more than one microstate to be compatible with our description, can we give meaning to the word ' possible ' (p. 93).

(The introduction of the term ' microstate ' seems gratuitous, for further references to underspecified situations deal with macro-objects, and Professor MacKay discusses only classical, not quantum, physical situations.) The importance of *our* assignment of a description is borne out by the statement that some situations ' can be made to manifest goal-directed features by suitable underspecification . . . [at] one level or mode of specification . . . while at another level they become totally meaningless' (p. 94). We are also told: 'It is not the physical situation that possesses a range of possible states, but our specification of it—if sufficiently open' (p. 94). (*b*) Second, underspecifiability also seems to be a function of the completeness of the description that *can* be assigned to an object. An apparently underspecified situation, a two-way switch, says Professor MacKay, does not have two possible positions, one open and one closed, for only one position, the one it has, is 'compatible with its past history' (p. 94). We could in principle describe the switch so completely as to rule out any possibility that it could be other than it is. For underspecifiable situations, such complete descriptions are not possible.

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[1] D. M. MacKay, ' The Use of Behavioural Language to Refer to Mechanical Processes,' this *Journal*, 1962, **13**, 89–103

Under interpretation (*a*), the underspecifiability of a certain two-way switch implies the possibility of describing it as 'S' ('that switch'), 's' ('that open switch'), or '$\bar{s}$' ('that closed switch') depending upon the degree of specification to be adopted. Interpretation (*a*) means only that for an underspecified situation there are several possible descriptions. Interpretation (*b*), on the other hand, seems to require that an underspecified situation be one for which there can be no complete description 'S' such that the conjunction of either s or $\bar{s}$ with S is a tautology.

Professor MacKay's contention that [a person's] 'bodily state is always and *necessarily* unspecifiable in full detail' (to himself) (p. 96) clearly demands interpretation (*b*), which is also implicit in the contention that the action of an object 'becomes irreducibly *under-specified* through its coupling with the rest of his [a person's] own activity' (p. 97). However, the notion of a 'commitment implicit in dialogue', demanding that we be 'rationally able and willing to adopt the appropriate standpoint of interactive relationship' (p. 96), seems to demand interpretation (*a*) of underspecifiability.

There seem then to be two forms of underspecification. Type (*a*), which we might call emotional (as it depends upon a commitment) or volitional (as it depends upon a choice), and type (*b*), which is logical. Logical underspecifiability is regarded as a logical consequence of interactions involving persons:

> Insofar as two people interact in dialogue and thus become one system, the crucial condition is *necessarily* fulfilled, since for each individual, his own (necessarily underspecified) situation becomes a determinant of that of his interlocutor, which thus shares the underspecifiability-to-him (p. 95).

Logical underspecifiability may be extended, Professor MacKay continues, to any system composed of an object interacting with a person. A golfer will find that his ball 'becomes irreducibly underspecified through its coupling with the rest of his own activity' (p. 97). Before the game, the golfer can describe his ball by a complete term 'G' such that any true partial description 'g' is deducible from G. When the ball is in play, there is no such term 'G', but only a series of partial descriptions, necessarily incomplete. After the game, the golfer finds 'the thing is now fully specifiable to him, and its temporary personal quality, dependent on the underspecifiability acquired through involvement, has vanished' (p. 97). Logical underspecifiability, then, is a necessary consequence of 'interaction' or 'coupling' and means that the agent cannot completely describe any object while he is interacting with it. The emotional form of underspecifiability is not necessary, however, but 'acquired' when we are 'willing to adopt the appropriate standpoint'.

It would seem that one or the other of these concepts of underspecifiability should have been rejected by Professor MacKay, but as we shall see, he has reasons for keeping them both.

Professor MacKay then considers under what circumstances it might be 'absurd' for someone to be 'willing to adopt the appropriate standpoint of interactive relationship'. Obviously, if underspecifiability is a *logical* consequence of any interaction between a person and some object, it can never be established that it is absurd to regard the interacting object as having personal characteristics. Also, under the logical view, the indefiniteness of the term 'interacting' is exposed. If the object is a golf ball, it is underspecified only at the moment it is struck, when the human portion of the man-ball system does not know where his stroke will direct it? Is it underspecified when it lies out of his sight in the tall grass at the edge of the fairway? Or all during the game, but only then? If 'underspecified' means only that the man cannot, at a given time, give a complete description of the ball, clearly the ball is underspecified to him at nearly all moments of its history.

However, if the logical view is not adopted, and instead some sort of emotional reaction, involvement, or commitment is considered the determinant of underspecifiability, it can easily be seen, as Professor MacKay points out, 'that at the cost of sufficient " detachment " even the personal significance of human activity (at least of other people's!) can be made to disappear, from a standpoint which views it as fully-specifiable-in-principle' (p. 97).

Getting rid of the conflict in what I have called the logical and emotional views of underspecifiability by getting rid of one or the other of them would have many undesirable results. Suppose the emotional view is done away with, and we forget about 'appropriate standpoints' and 'willingness to become involved'. Now it will become possible to over-populate the universe, personalising any object for which one is unable to give a fully-specified description: at the very least, any object with which one interacts in some vaguely-defined fashion. Suppose we get rid of the logical view. Then it becomes possible to depopulate the universe completely, leaving only oneself with personal characteristics defined by necessary unspecifiability.

To evade both undesirable alternatives, Professor MacKay has recourse to a 'linguistic community' of persons, who can confer upon or deny to objects the status of being 'one of us'. This community has certain criteria for awarding personal status; ability of an entity to 'converse intelligently and purposefully about his own activity and experience' (p. 100), seems the most important. But this criterion can be seen to be circular in Professor MacKay's scheme. 'Purposefully' (the key word, since 'intelligence' is never defined) means for Professor MacKay only that any situation described by purposive language must be underspecified. Hence, the linguistic community may legitimately regard as purposeful (underspecified) the behaviour (or conversation) of objects with which its members interact, for underspecification is acquired in the interaction. The criterion (if logical underspecification is used) remains too general, or (if emotional underspecification is used) reduces to a mere attitude of the arbiters.

Professor MacKay would like to rule out the possibility of a decision to accord personal status to entities 'as a matter of convention', or based only on behavioural criteria. 'Consciousness is a matter of fact, on which we are either right or wrong' (p. 103), he says, but he couples this indisputable (since not precise) statement with the assertion that the ' matter of fact ' is not to be settled by appeal even to observable evidence, much less to ordinary language or theology. Behavioural criteria ' may indeed be inevitable ' but ' the simple truth is that we cannot know ' (p. 103).

What could be the source of this doubt? Possibly it arises from a belated recognition of the irreconcilable apparent alternatives (neither tenable) that everything is a person or else nothing is. But there are indications that the ' ontological doubt ' has another source, which gives rise as well to the two untenable notions of underspecifiability as a criterion of personality. If all observable and behavioural criteria for being a person are satisfied by an entity, and ' doubt ' remains about whether that entity is, in fact, a person, this doubt can only mean that the criteria are regarded as signs of the presence of ' something more ' which makes that entity a person. By definition, this ' something more ' is unobservable in principle. Initially, Professor MacKay seems to regard this ' something more ' only as the necessary underspecifiability which each person has with respect to himself and is willing (or forced) to extend to others in dialogue with them. Then since some degree of underspecifiability is gained by any object interacting with a person, the notion of ' commitment implicit in dialogue ' is introduced. While a person can choose to withold this commitment by refraining from dialogue, there is no other way he can do so. But in his conclusions, Professor MacKay seems willing to admit the possibility of an unobservable ' something more ' over and above the difference in viewpoints when a person describes himself ' from inside ' and when he describes another, whom he sees ' from outside '. It would seem that if an entity meets the observable criteria, whether the entity is called a person because the ' arbiters ' are willing (emotionally committed) or forced (because it is a logical necessity), there is no room for an unobservable something more, whose presence or absence must then be further considered.

The ' ontological doubt ' seems to be introduced by the same factor which splits underspecifiability into two different concepts. This factor is the unclearness of the notion variously represented by ' commitment ', ' dialogue ', ' interaction ', and ' becoming one system '. A clarification of this notion is needed to prevent persons (whether men or machines) from becoming bodies haunted by ghosts of ' something more ' perambulating about our universe of discourse. Presumably, such a clarification would indicate a difference in kind between the ' dialogue ' of two men conversing and that of a golfer and his ball. This might eliminate the need for recognizing persons by Professor MacKay's ' curious blend of logic and commitment '—which can produce some curious results.

P. J. Fozzy

Consciousness and Mechanism : A Reply to Miss Fozzy

Miss Fozzy's summary of my argument is simply incorrect. My first necessary condition for the recognition of personality in natural human beings was that ' the physical " bodily situation " must admit of and support in practice an underspecified state-description *showing appropriate behavioural features: the ' observable indications ' of thinking, feeling, hoping, fearing etc* ' (p. 96, italics added). By ignoring the qualifying condition, Miss Fozzy has led herself into the confusion which she attributes to me.

Thus the ' undesirable alternatives ' which she accuses me of ' evading ' (that ' everything is a person, or else nothing is ') are simply mis-deductions, which could be made plausible only if underspecifiability had been proposed as a *sufficient* condition for 'personalizing' objects. Even for my irate golfer, whose whimsical defence may have been misunderstood, I could plead only that '*insofar as* (*the balls*') *activity is a kind of distorted reflection of his own*, to revile it in personal terms is not perhaps totally absurd . . .'.

The distinction which I drew (on p. 95) was between (*a*) situations which merely *admitted* of description in underspecifying terms, and (*b*) situations which necessitated it, in the sense that no fully-detailed specification was possible. Since my first two examples of (*a*) were a 2-way switch and a thermodynamic system, it seems a little odd to call the resulting underspecifiability 'emotional'; but in any case Miss Fozzy does nothing to show the existence of a '*conflict* in . . . the logical and emotional views of underspecifiability' (my italics), still less that one or other possibility 'should have been rejected'.

If she means 'rejected as a *sufficient* condition for recognition of personality', then of course I agree; in fact the sentence on page 94 quoted incompletely by Miss Fozzy reads: 'While *not every situation* can be made to manifest goal-directed features by suitable under-specification, it is quite possible for one level or mode of specification to bring out such features while at another level they become totally meaningless' (italics added).

A clue to the misunderstanding here may be given by her description of a 2-way switch as 'apparently underspecified'—an expression (attributed to me) to which I attach no meaning. She goes on: 'A 2-way switch, says Professor MacKay, does not have two possible positions'. What I said (p. 93) was 'A 2-way switch *is a device with two possible states* . . . . But *this* "2-way switch" is a *physical array of molecules* in the *only* state compatible with its past history' (italics added). In other words, I was contrasting what can be said about '*a* 2-way switch' (which *does* have two possible positions) and '*this* . . . array of molecules' which (on Newtonian assumptions) does not. This is an optionally 'underspecifiable' situation. We may take it as 'a switch' or as 'molecules'. Whether or not it is in fact 'underspecified' depends on who does the specifying, but in either case it would be odd to say that it is only 'apparently' underspecified.

By contrast, the bodily states of human beings in dialogue can never be fully specified by or to one another, so they are 'necessarily underspecified' *for them*. People in dialogue can optionally adopt modes of description which leave one another's bodily states still further underspecified—as we commonly do. They cannot however remove the minimum degree of underspecification (which arises basically from the existence of 'interaction terms' in their joint state-description) except at the cost of totally breaking the dialogue so as to eliminate the reciprocal interaction.

Miss Fozzy is, of course, right to point out that the kind of interaction we call 'person-with-person' needs more examination. I have made some efforts in this direction in references given in my paper and elsewhere [1] which should at least dispose of the extraordinary statement that 'Purposefully . . . means for Professor MacKay only that any situation described by purposive language must be underspecified'.

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[1] D. M. MacKay, 'The place of "Meaning" in the Theory of Information'. *Information Theory* (E. C. Cherry, ed.), Butterworths, 1956, 215-25

D. M. MacKay, 'The Informational Analysis of Questions and Commands'. *Information Theory* (E. C. Cherry, ed.), Butterworths, 1961, 469-76

D. M. MacKay, 'Communication and Meaning—a functional approach' in *Epistemology in Anthropology* (Helen Livingston, Ed.) Harper & Row, 1963. (In Press.)

But nothing she says seems to justify begging the question whether an entity that behaves-as-if-conscious *ipso facto* is conscious. Her criticisms leave unanswered my final contention, which is that although (1) a sufficiently isolated onlooker can never be compelled by behavioural evidence, however typical of 'personality', to recognize the need for personal categories (since for him (*ex hypothesi*) a fully-specified mechanistic description and explanation of the behaviour is available in principle), whereas (2) if he enters into dialogue, the situation now defies complete mechanistic specification and explanation by him, so that he has no refuge at that level from the personal interpretation which we suppose the behaviour to support, nevertheless (3) to persuade him to recognise the personal significance of behaviour does not logically oblige him to admit that he is in the presence of a conscious being—conscious in the sense that he is himself. 'Personal' behaviour may or may not be a necessary condition of consciousness; it certainly remains to be proved that it is a sufficient one.

D. M. MacKay

REVIEWS

THE FACTORISATION OF ENERGY

Professor Hiebert's proclaimed aim in his recent work[1] is to trace 'the historical roots of the principle of conservation of energy within the framework of mechanics' prior to the middle of the eighteenth century (pp. 2-3, 4, 95). One of the major difficulties encountered in the study of the historical development of a scientific concept is the question of the unambiguous identification of the concept in the source material of periods prior to the time when the concept was finally given its currently accepted name. The conception of energy, for instance, played undoubtedly an important role in scientific thought long before John Bernoulli introduced the term 'energy' 1717 in his letter to Pierre Varignon of the Paris Academy of Science and long before Young, Thomson, and Rankine promulgated in their writings the word as a *terminus technicus* in its modern sense.

Professor Hiebert attempts to solve this difficulty by enunciating a general formula which 'adequately defined mechanical work as the product of a capacity factor (weight or force) and an intensity factor (displacement). This was deemed necessary in view of the fact that a mathematical concept of work was being analysed which was considerably older than its name' (pp. 95-96).

Since such recourse to the mathematical structure of a physical concept as an expedient for its identification during its formative history may be thought of importance not only for the methodology of historical research but also the philosophy of science in general it is worthwhile to analyse this suggestion in more detail.

The 'factorisation-theory' of energy according to which energy is the product of a capacity factor and an intensity factor had its origin in an article written by Rankine in 1859 (*Phil. Mag.* (4) 17) in which the author distinguishes between two constituents in every process of energy-transformation: the magnitude of change and the strength of the tendency for change. This distinction was twenty-five years later revived by Joseph Popper in his *Physikalische Grundsätze der elektrischen Kraftübertragung* (Leipzig, 1884, p. 8 ff.) and played eventually an important role in Wilhelm Ostwald's 'energetism' (*Vorlesungen über Naturphilosophie*, second edition, Leipzig, 1902, pp. 281 ff.). The deeper reason for the inclusion of this bifurcation in the late nineteenth-century theories of energetics—the factorisation *prima facie* seems to contradict the contended primacy of energy as the ultimate not further analysable substratum of physical reality—lies in a convergence with another conceptual development: the differentiation between extensive and intensive magnitudes. Originating in Aristotle's discussion on condensation and rarefaction (*Physics*, Book IV, chap. 9, 217 a 32), elaborated by Simplicius (*In Categ. Comm.*, chap. 8) and later by Augustinus for theological considerations (*De trinitate*, Book VI, chap. 7) this differentiation led in scholastic natural philosophy to the distinction of increase of qualities either *secundum extensionem* or *secundum intensionem* and subsequently played an important role in the *calculationes* of the fourteenth century. With Huygens's insistence on the intensive 'dureté parfaite' of matter in his polemics against the purely extensional Cartesianism modern classical physics reinstated this difference which eventually at the end of the last century became a subject of much philosophical discussion (Paul de Bois-Reymond, H. v. Helmholtz, Wundt, A. Höfler, J. Bergmann, P. Boutroux). The recognition that the concept of energy constitutes a merging or amalgamation of such disparate fundamental categories of physical manifestations seemed to excel it and to accentuate its unique status in physical theory.

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[1] Erwin N. Hiebert, *Historical Roots of the Principle of Conservation of Energy*, The State Historical Society of Wisconsin for The Department of History, University of Wisconsin, Madison, 1962, 118 pp. $3·50

The adoption of this factorisation as a methodological device, as suggested by Professor Hiebert, raises however a number of serious questions. First of all it is not always clear which of the two factors involved stands for the capacity or extensive quantity and which for the intensive one. According to Hiebert, for example, the force F in the expression for mechanical work, $\int F\, ds$ (ds is the element of path), is a capacity factor (p. 96) while Georg Unger (*Vom Bilden physikalischer Begriffe*, Stuttgart, 1959, part I, p. 55) states explicitly in a similar analysis of the concept of mechanical work that force is the intensive quantity. True, Helmholtz in his *Zählen und Messen, erkenntnistheoretisch betrachtet* (Leipzig, 1887), considers, like Hiebert, force as a capacity, but from the context it is clear that Helmholtz regarded it as a volume integral of force density. Similar examples for the ambiguity of the definitions involved may be quoted. The dichotomy of physical quantities into extensive quantities answering, according to Arnold Sommerfeld, on the question 'How much?' and intensive quantities answering on 'How strong?' (*Elektrodynamik*, 1948; English translation, *Electrodynamics*, New York, 1952, p. 11) leaves much room for ambiguities. This, of course, is not to ignore the clear-cut difference between mass-depending extensive variables and mass-independent intensive variables as used in thermodynamics.

In order to clarify the situation a more philosophical analysis of the concept of energy is called for. Speaking from the ontological point of view, one may say that modern physics has raised the status of energy from a mere accident of a mechanical system to the autonomous rank of an independent existent. Two circumstances were responsible for this process: (1) field theoretical methods concerning radiation phenomena had to consider distributions of energy in space independent of corresponding distributions of matter in the sense of the classical corpuscular-kinetic theory. The notion of energy transmission, introduced originally by Coriolis in his *Traité de la mécanique* (1829; § 59) in a purely mechanical sense, gradually lost its mechanical connotation through the work of Poncelet, Maxwell, Poynting, Poincaré, Mie, and Einstein. Instead of being conceived as 'transported' by material vehicles it had eventually to be postulated as 'flowing' on its own through space without the guidance of any material substratum. (2) Through its relativistic unification with *mass* energy also lost its classical indeterminateness as far as an additive constant was concerned and thus became a physical quantity of absolute magnitude. Philosophically the ground for this conception of energy had been well prepared by the school of energetics.

From the epistemological point of view the universal function of energy can be equally well accounted for by a relational interpretation according to which the concept constitutes an integrating principle: it establishes among heterogeneous natural phenomena, such as heat and motion, a mutual comparison and to some extent an interchangeability in the causal chain of events. Instead of co-ordinating each individual process of a certain category of phenomena with the multitude of corresponding equivalents a common numerical scale is constructed the unit of which, *qua* unit of energy, serves as a common denominator for comparison. In principle, the choice of the standard is completely arbitrary; its replacement by a different unit does not affect the result (just as in the relational theory of time the angle of terrestrial rotation as the standard for comparison of time intervals can be replaced by any other periodic process). 'The fact that mechanical work was chosen historically as the common denominator seems plausible in view of our knowledge that, on the one hand, mechanics was the oldest and most investigated branch of physics, and on the other, that mechanical effects are more easily measured than other energy effects' (p. 5). In fact, modern atomic physics employs in general the *electron-volt* (eV) and not the *erg* as the common denominator. According to Robert Mayer, Georg Helm, and especially Ernst Cassirer who propounded the relational conception of energy, the postulation of the existence of such a one-to-one coordination exhausts the essential contents of the principle of conservation of energy, since the assumption of 'energy-creation' would clearly violate the principle of coordination.

Finally, from a metaphysical point of view, energy has been interpreted as an expression of the principle of causality. Paradoxically, the anti-metaphysician Ernst Mach has to be reckoned as one of its chief advocates. In his *Geschichte und Wurzel des Satzes von der Erhaltung der Arbeit* (Prague, 1872, p. 46) he declares: 'I believe to have shown that the theorem of the excluded perpetuum mobile is only a particular form of the law of causality'. (Cf. also his *Principien der Wärmelehre*, second edition, Leipzig, 1900).

Considering what he calls 'the logical root of the theorem' Mach ignores the fact, accepted both by rationalists and empiricists, that there are no sensuous impressions corresponding to the concepts of cause and effect.

Professor Hiebert undoubtedly professes the relational interpretation when he proclaims (p. 1) that 'the principle represents a convenient intellectual point of view which permits energy phenomena to be organized under one system provided they can be compared linearly along a numerical scale with a single energy denominator serving as a standard of reference. . . . This hypothesis exhausts the essential meaning of the principle of conservation of energy'. 'The real meaning and function of the energy concept lay rather in the abstract mathematical relationships which could be established between the various forms by means of a single general equation' (p. 3). On philosophical grounds, Hiebert's relational conception should have restrained him from commending Ostwald's factorisation principle.

The situation becomes clear if we consider, in addition to the substantial, relational and causal conceptions of energy mentioned so far, the new insight gained in this respect by modern science through Emmy Noether's famous theorem in mathematical physics (*Invariante Variationsprobleme*, Goettinger Nachrichten 1918, 235 1–57). It holds a key position in the historical development of physical theory which may be characterized by the shift from 'equations of motion' (like Newton's second Law) to 'conservation rules' (Mayer, Helmholtz) and from here to 'rules of invariance' Einstein) as the first principles of physics. Each stage in this development not only comprised the previous one but generalized it and clarified its meaning. The question of how to derive the laws of Newtonian dynamics from conservation principles was one of the major issues of the Conference of Scientists held 1895 in Lübeck (cf. for instance, J. R. Schütz, *Prinzip der absoluten Erhaltung der Energie*, Goettinger Nachrichten 1897, 110–23). Noether's theorem, on the other hand, demonstrates the conservation of certain quantities (e.g. the canonical energy-momentum tensor) for dynamical systems which are invariant under continuous transformation groups (of the co-ordinates and/or field functions involved). In particular, if homogeneity of space and time is assumed, that is, if the invariance of the system under translational transformations of the origins of space- and time coordinates is postulated, the conservations of momenta (quantities of motion) and of energy is merely a mathematical deduction. The principle of energy is therefore ultimately a consequence of the invariance under changes in the zero-point of the time-scale, i.e. of the homogeneity of time. Although these conclusions are of great importance for the philosophical assessment of the energy principle since they seem to relate it again, to some extent, to the principle of causality (if the essence of the latter lies in the assumption of the homogeneity of space-time), and thus deserve a more detailed examination, we shall confine our discussion to their bearing on the specific problem of

Hiebert's methodological device. As can be shown by an exact mathematical analysis the term responsible for the Ostwald factorisation of energy is in the Noether derivation, apart from the Lagrangian *L*, an expression of the form $\frac{\partial L}{\partial \partial_\mu \psi_\alpha} \partial_\nu \psi_\alpha$ where ∂_ν denotes the partial derivation of the field variable ψ_α with respect to the co-ordinate x_ν. Since the physical nature of the ψ_α is not determined—they may be ordinary or generalised co-ordinates as for mechanical systems with a finite number of degrees of freedom, or electromagnetic potentials as in electrodynamics, or functions of a much more complicated transformation character as in the modern theory of elementary particles—the Ostwald factorisation theory has no scientific justification. In this respect it was only of minor importance when E. Meyerson in his *Identité et Réalité* (5th edition, Paris, 1951, pp. 396-401) exposed the vulnerability of Ostwald's thesis for the case of heat energy with its capacity factor of entropy.

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The preceding remarks tried to expose the inadequacy of a methodological device such as suggested by Professor Hiebert for the tracing of the general conception of energy in its early phases of development. In general, one must add, most of the fundamental conceptions in modern science possess a long history of formation (space, time, mass, force, etc.) prior to their exact quantitative formulation. In such cases, obviously, recourse to the mathematical structure of the concept would be of little avail.

Although the introductory and concluding chapters of Professor Hiebert's *Historical Roots of the Principle of Conservation of Energy* present a detailed discussion of the above-mentioned device, little use of it is made in the text itself. For, contrary to the title of the book, it is not *energy* but *mechanical work* and its conservation which forms the subject of the study. And here the notions of force and displacement (virtual displacement or virtual velocity) are explicitly formulated in almost all sources referred to. In reviewing the book as a whole one certainly has to deplore a certain laxity and vagueness of expression, especially as far as the mathematical terminology is concerned. In connection with the above-mentioned methodological device the author, for example, speaks of 'the infinitesimal energy change, *dE*' (p. 3) 'owing to the absence of any way of calculating absolute energies', while he should obviously refer to finite energy differences $\triangle E$. Looseness of expression leads him to equations of the type $dE = {}_{x}^{y}\int CdI$ or to the statement: 'When the displacement is not in a straight line and the force is not constant the mechanical work done for an infinitesimal displacement *ds* along any path *xy* is given by $W = \int_{x}^{y} Fds$' (p. 7), or even: 'When force is summed (integrated) over an interval of time the result is energy (work)' (p. 107).

The book itself, however, apart from the 'Introduction' (pp. 1-6) and the chapter entitled 'Conclusions' (pp. 94-107) which were the object of the preceding criticisms, is a thorough and competent analysis of the growth of the concept of mechanical work and the notion of quantity of motion. Chapter One, almost two-thirds of the work, traces the former concept from its early inception in antiquity to its mature formulation in Lagrange's *Mécanique Analytique* (1788) and shows how the law of the lever, in the early studies of the action of simple machines, formulated at first in terms of virtual velocities, later in terms of virtual displacements, was the first extant attempt to analyse the science of mechanics on a rational theoretical basis. In his systematic application of the principle of virtual work to a great variety of practical problems Hero of Alexandria makes an implicit use of the notion of mechanical work. This discussion is followed by a detailed description of the statements of the law of the lever in the medieval 'science of weights' ('scientia de ponderibus'), in the *Liber Karastonis*, the *Liber de Ponderibus* ascribed to Jordanus de Nemore whose notion of 'positional gravity' ('gravitas secundum situm') in the derivation of the law of the lever and in the correct solution of the problem concerning the equilibrium condition of two weights on opposite sites of diversely inclined planes paved the way to the conception of potential energy. Leonardo's, Cardan's, Ubaldo's and Stevin's contributions to these problems are critically analysed. Galileo's application of the principle of virtual velocities to the Roman balance, the lever, greared wheels, the windlass, the pulley, and the screw as well as the elaboration of this principle by Descartes, Torricelli, Pascal and its synthesis with the notion of the common centre of gravity by Wallis are thoroughly examined. The chapter concludes with a discussion concerning the generalisations and refinements of the principle introduced by Bernoulli, d'Alembert, Lagrange, and Poinsot.

Chapter Two discusses the seventeenth-century attempts to correlate the 'quantity of virtual work' (later called 'potential energy') stored in a body due to its gravitational position with the quantity of motion acquired by the body by falling from this position. Galileo's contribution to this problem in his *Discorsi* (1638), Descartes' contention of the conservation of the quantity of motion in his *Principia Philosophiae* (1644) and the subsequent developments by Gassendi, Newton, Toland, Wallis, Huygens, Leibniz, and Bernoulli are presented to the reader together with numerous quotations from the sources. Professor Hiebert has undoubtedly studied the original texts and did not merely rely on secondary sources. Thus, to mention only one example, in his treatment of Spinoza's exposition of the Cartesian conservation law the author refers to the *Treatise on God, Man and his Wellbeing* in which the philosopher from Amsterdam 'says that two things—motion in matter and the understanding in the thinking thing—have existed from all eternity and will remain immutable to all eternity'. Hiebert, contrary to most secondary sources on this matter (cf. e.g., A. E. Haas's *Entwicklungsgeschichte des Satzes von der Erhaltung der Kraft*, Vienna, 1909, p. 11), adds: 'Note that he excludes matter'. An examination of the Dutch original confirms Hiebert's statement.

Chapter Three (pp. 87-94) supplements the previous discussion for the case of inelastic collisions where mechanical energy seems *not* to be conserved. Gassendi, Borelli, Leibniz, and Wolff maintained the conservation principle by contending that the motion is transferred from the colliding macroscopic bodies during their impact to their internal invisible parts, while John Bernoulli and s'Gravesande saved the principle by postulating latent forces of compression among the elastic parts of the bodies. These considerations naturally led to the notion of a generalised potential energy and raised the problem of the relationship between motion and heat, a problem beyond the scope of the present book.

The exclusion of the thermodynamics of the nineteenth century and the systematic omission of general philosophical considerations whose impact on the final conception of the energy principle cannot be ignored (Leibniz, Wolff, Mayer, Joule, Colding, Mach, Ostwald, etc.) restrict the treatment to the conservation of mechanical work. As a history of the concept of mechanical work and its conservation Hiebert's study is a valuable addition to the literature on the history of mechanics (Dugas, Moody, Clagett).

MAX JAMMER

The Concept and the Role of the Model in Mathematics and Natural and Social Sciences. Proceedings of the Colloquium sponsored by the Division of Philosophy of Sciences of the International Union of History and Philosophy of Sciences organised at Utrecht, January 1960.
D. Reidel, Dordrecht-Holland, 1961, Pp. 194.

THE articles in this volume may be divided into three classes. There are three articles, by L. Apostel, P. Suppes, and J. B. Ubbink, which are devoted to a survey and general discussion of the concept of model. Then its use in mathematics and logic is described in the five essays written by R. Feys, R. Fraïssé, A. Grzegorczyk, M. Guillaume, and J. F. Staal. The remaining nine papers are concerned with the application of the concept of model in empirical science. E. W. Beth, J.-L. Destouches, H. Freudenthal, G. Frey, H. J. Groenewald, A. Kuipers, P. X. Yêm write about physics and applied mathematics. R. C. Atkinson discusses the model in experimental psychology and J. J. Mulckhuyse in chemistry.

The model was added to scientific methodology by the nineteenth-century physicists. The phenomena of electromagnetism and of light require the concept of field for their description; and this conception goes beyond the mechanistic theories known until that time. Thus the *aether* was introduced as a model in order to link the more 'abstract' field to ordinary mechanics. The aether disappeared once we learned to think on the level of relativity theory. But the model reappeared when the next step in 'abstraction' had to be taken, namely, when quantum theory began. Plancks's model of the oscillator, the planetary model of the atom, etc. were needed so as to relate the electron's behaviour inside the atom to the known laws of electromagnetism and mechanics. The Bohr-Rutherford model was superseded by the vector model and the wave-mechanical model of the atom; and so it has been going on ever since. This historical origin of the concept has to be kept in mind, I think, if we want to do justice to it and keep the discussion within reasonable limits.

The meaning of 'model' then ranges from material representation to symbolic interpretation; it is this latter sense that has gradually become dominant in modern physics and in science in general. Mr Apostel discusses the algebraic, syntactical and semantic concept. Approximative isomorphism characterizes the first; the second is Tarski's and Carnap's formal interpretation of a calculus; and the partial interpretation of a language system in terms accessible to experimentation is the third concept. This is called the *approximate constructive necessary satisfaction* by Mr Apostel. It is the semantic concept that is of main import to science; differing quite distinctly from the two other concepts, it is the rational reconstruction of the term used by physicists.

This comes out clearly in Suppes's article comparing the various meanings of the concept. Tarski's statement is, 'A possible realisation in which all valid sentences of a theory T are satisfied is called a model of T'. Although he quotes a wide range of other uses, Mr Suppes is attached mainly to the syntactic concept. For he takes the *Gedanken* experiment as exemplifying a model while, semantically, it is the very opposite. Such an experiment illustrates a situation in terms that are not experimentally realisable (though theoretically allowed). The sematic concept, however, provides an incomplete (or partial) interpretation of a new theory in terms of the old; thus a more comprehensive and 'abstract' theory can be related to experiment.

In the last of the general essays, Mr Ubbink gives a philosophical discussion of the relation between model and the knowledge obtained by its help. Rejecting the traditional view that observation and description are 'unimpeachable', he sees the model as an instrument towards a more imaginative approach.

The next group of articles concern logic and mathematics and are severely technical; some of them are not even clearly referring to the concept of model. There is an interesting attempt by M. Guillaume of bringing together the syntactic concept and Gödel's relative version of the axiom of choice. Staal's essay on formal structures in Indian logic provides out-of-the-way material, though I doubt whether anything in this logic can be called ' formal ' in the western sense.

The application to science is discussed in the major part of the contributions to this colloquium. Beth writes briefly of the semantic structure of quantum theory. Destouches describes the hydrodynamic model in wave mechanics. Frey speaks about symbolic and iconic models; both matrix mechanics and wave mechanics are regarded by him as non-iconic, symbolic models of an abstract formalism. Groenewald distinguishes between representative, substitute and study models. The one-to-one correspondence between abstract formalism and observational picture is lost in quantum theory; there are logical operations in the formalism that have no counterpart in observation. Thus particles and waves and the various kinds of ' basic ' fields (of electrons and photons), though retaining some of their fundamental meaning, become substitute models in advanced theories. According to Kuipers, the model allows us to grasp the essential facts of a physical process while, at the same time, it arises from simple experience; it is therefore a tool for gaining insight. Yêm speaks about models of interaction between particles; since particle theory is too ' abstract ', intermediate models, already somewhat formal and mathematical, are needed to connect it with experiment. Freudenthal argues that there are two models in applied probability. The first is the urn model thought to be indispensable by most statisticians. It is characterised by the three postulates of (i) the constancy of probability distribution, (ii) the random character of choice, and (iii) the independence of successive choices. The second model is that of the complete strategist as exemplified by Wald's decision theory. Thus we obtain the minimax strategy—a strategy that is best under the worst possible circumstances. Both models imply a special view of human nature as well as a particular form of randomness (e.g. whether Tippett numbers or a Turing machine are chosen to represent it). A good deal of futile dispute could be avoided, I think, if people would understand that the application of probability calculus depends on models containing an essential (semantic) weakness.

Finally, Atkinson gives a model for learning theory which is formulated in six axioms. The model is generally characterised by saying that ' (i) associated with each response alternative there is a tendency to approach or avoid that alternative and (ii) the response which is finally made on a trial depends on the observing or orienting behaviour of the subject in the predecision period of the trial '. It is clear that such a model of human behaviour is strictly limited even though it provides a neat, mathematical formulation (Markov chains). In chemistry, the models of a molecule have long been in existence; they vary fron the simple structural representation of Kekulé to Lewis's octet of electrons and Pauling's wave-mechanical resonance. Mulckhuyse constructs an axiom set for the chemical bond from which he derives a formal, mathematical model theory. The theory is, in my view, too abstract though restricted—it is even *undecidable* in the Gödelian sense—to be (semantically or pragmatically) successful as a model.

There are valuable ideas to be found in this book. My general criticism is that some of the articles are too long—they would be better listened to than read—and some are too short and technical. To move a whole mountain of symbolism in order to find a little mousy bit of knowledge underneath it is a bit strenuous. Perhaps this is unavoidable when people of different nationality and background contribute to an international colloquium. What is avoidable, however, is the provincialism—if not to say parochialism—of the contributors. Every writer quotes only himself or the work of his compatriots; at best, only classics in a language other than that of the author are mentioned. To put the record straight I want to mention that the semantic concept of model in physics was first introduced in my paper entitled *On Semantics and Physics* (published in the *Proceedings of the Aristotelian Society*, 1948-49). The second paper on the subject, *The Role of Models in Physics*, was originally given as a lecture in June 1953 (published in this *Journal*, 1954, 4). It seems we need a European Common Market for ideas more than for material goods. Certainly the history and philosophy of science should be as international as science itself.

E. H. HUTTEN

The Dignity of Science: Studies in the Philosophy of Science presented to W. H. Kane, O.P. Edited with introduction by James A. Weisheipe, O.P.
Thomist Press, 1961. Pp. xxxiii+526. $6.

'ONLY an unreasonable or prejudiced thinker would dismiss this wisdom of the ancients [the *philosophia perennis*] without fair study' writes Weisheipl in his introduction to this collection (p. xxiii). I have for long been concerned about my lack of contact with medieval scholastic and modern Thomist thought, and I took up this book in the hope of finding enlightenment and new understanding.

This collection of papers is designed to mark the tenth Anniversary of the Albertus Magnus Lyceum, and half the contributors are Dominicans. Under the leadership of Weisheipl, one would expect a stimulating view of the new Thomism, and its bearing on scientific thought. But the actuality does not meet these expectations. Those who reject the Aristotelian view of science, according to which science is the arrival at certain conclusions starting from principles which the human intellect grasps with certitude—more, those who reject the idea that any factual discipline falls under this description—will not find discussions to resolve their basic difficulties.

There are a few pages in which the contemporary philosophical scene is discussed (in McNicholl's 'The challenge to the traditional ideal of science'), but the author here lacks understanding even of the facts of the contemporary scene,[1] (though he writes in such a way that his fellow Thomists will not fail to be impressed by the profundity of his understanding) and his real purpose is to expound the 'Tasks for Thomists'. In another paper we are enjoined to 'shake off the Wolff-Leibnizian influence and discontinue presenting natural philosophy as an application of metaphysics' (p. 273). Nowhere do the authors come to grips with modern thought.

Perhaps we should recognise that the contribution of Thomism is still most valuable in the field of historical research. The modern Thomist has a natural grasp of the scholastic view, and this is well illustrated in a paper by Weisheipl which analyses the thirteenth-century views concerning the celestial movers. He uses as his main texts the answers given by Kilwardby, Albertus Magnus, and St Thomas to a set of questions asked by the Master General of the Order of Preachers in 1271. Weisheipl brings out the difficulties found by the medieval mind, both of the view that the circular motion of the celestial bodies is caused by their own nature, and of the view that it is caused by angels; and his discussion is particularly illuminating, for he makes the medieval mind come alive, with its preconceptions comprehensible by us. Other papers also illuminate Medieval mechanics, and its philosophical foundations, in one way or another. Another historical paper which should be mentioned is Hoskin's survey of Clarke's annotations to Rohault's *Physics*; this is useful scholarship, but does not impinge on philosophical difficulties.

Not all the historical contributions, however, are of quite the quality of Weisheipl's. Sound history of science demands a sympathetic understanding both of the framework into which the subject fits, and of the framework which emerged subsequently; and this ambivalence is difficult to achieve when a gulf, such as opened in the seventeenth century, separates the two frameworks. The difficulties are well illustrated in Ratner's paper on William Harvey. Ratner presents Harvey as an explicit devotee of Aristotle and his method, and analyses his discussion of the motion of the heart as a classic Aristotelian demonstration: 'the innovation of Harvey lay in . . . fidelity to tradition . . . and establishes him as a model for an age that slights sense [i.e. the fifteenth-sixteenth-century caricatures (p. 71) of Aristotle], as well as for an age that slights reason [i.e. us]' (p. 72). Ratner points out how standard translators and commentators are seriously defective in their understanding of his background; but Ratner himself fails to appreciate the driving forces of 'modern science'. The paper makes sobering reading for anyone who is writing about the seventeenth-century 'discovery of scientific method'. One may well find that Ratner is pillorying a caricature of one's own sins (lack of perception of the other point of view), and at the same time perpetrating those same sins, reversed in historical perspective.

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[1] The most straightforward example: he seems to think that the axiom of choice enjoins one to seek only the more simple among many possible hypotheses (p. 458).

Taking the collection as a whole, it seemed to me that the predominant balance of the papers was historical rather than philosophical (not merely the 5 out of 22 characterised 'History of Science'). But there exists no clear distinction between the analyses of historic views, and the analysis of philosophical (or scientific) problems; the two activities blend, particularly where a tradition is in the process of trying to re-vivify itself. It is interesting to try to pick out possible points of growth, and here Oesterle's 'The significance of the universal *ut nunc*' may be mentioned, for it broached old ideas newly. Thus: '. . . it is clear that the domain of universality *ut nunc* [" for the time being "] has far greater dimensions than that of true universals, and this is the point of emphasis in this paper, a point which seems to have been somewhat ignored in the scholastic tradition' (p. 36). 'What they [Democritus and Dalton] were ignorant of was the real import of what they said [about " atoms "]. . . . What we must recognise is that there can be uncertainty, not only as to whether B belongs to A, or . . . but that there can be uncertainty concerning what the term A exactly stands for' (pp. 30-31). 'That nature is, is *per se* known, in so far as natural things are manifest to sense. But what the nature of any thing is, or what its principle of motion, is not manifest' (St Thomas, quoted p. 29). Surely Oesterle is right in suggesting that here we have something neglected by the Scholastic tradition in the past, but which is of importance for its future. Yet, what is the future of Thomism ? I did not find much in this volume to inspire confidence. Yancey discusses 'American Catholics and Science'—and the picture is depressing. 'It should be abundantly clear that there is nothing in the tenets of the Catholic Church not consonant with scientific endevour' (pp. 506-507). Yet the record is so bad (see e.g. p. 505 where the proportion and quality of Catholic scientists is discussed) he is forced to ask 'Is there an inherent factor, in content or method, in Catholic education that stifles pre-scientific curiosity?' (p. 513). The authoritarian approach to theology, with the ultimate appeal to the word of God and His Church, does not imply an authoritarian approach to science, and the contemporary failure is not necessary. But the general inadequacy of the relation of Catholicism vis à vis science is reflected in the present volume. I exclude two papers on psychology and one on sociology, which I am not competent to comment upon: there remain three papers which refer to fundamentally scientific matters. Vincent E. Smith clearly does not understand his subject 'Evolution and entropy': as one particularly simple example, I will refer to his apparent confusion of the Hubble red shift and the sunset reddening (p. 314). Sister Margaret Ann 'The rhythmic universe' seems incredibly naïve. Nogar 'From the fact of evolution to the philosophy of evolutionism' understands his subject, but is still caught up on the web of unpleasantness, recrimination, and self-justification created in the Darwinian era.

We can all agree in seeking 'the possibility of educating human beings intelligently devoted to science, rather than technicians unaware of the dignity of their pursuit' (Weisheipl 'Introduction', p. xviii)—but how is this to be achieved? 'We expect Catholic philosophers and scientists to make positive contributions to science and its theoretical foundations . . . not because they might be more gifted, but because they have the advantage of the true faith and the resources of a *philosophia perennis*' (p. xxii). On the one hand we have 'the scientific optimism of Aristotle and the ancients' for whom 'science is the attainment of true and certain causes within reality. . . . Aristotle's lofty, and perhaps unattainable, ideal'; on the other 'modern theoreticians apparently have abandoned hope in the power of man's speculative reason; they seem to be content with universal uncertainty and a solitary path to knowledge' (p. xx). Where does the true dignity of science lie?

H. V. Stopes-Roe

The Story of Alchemy and Early Chemistry. By J. M. Stillman.
Dover Publications, New York, 1960. Pp. xiii + 566. $2.45.

This book, which originally appeared in 1924 under the title *The Story of Early Chemistry* and has long been unobtainable, has been re-issued as a paperback. The first 300 pages are devoted to the development of chemistry up to the sixteenth century and they provide a clear account of practice and theory in early technical chemistry and alchemy. The author provides a generous proportion of texts in translation. The sixteenth century is labelled 'progressive' and prominence is given to the influence of Paracelsus. Helmont is treated more summarily, although Stillman appreciated the enormous influence which Helmont had in the seventeenth century, not least on Boyle. The phlogiston theory is described in the usual way, but the short chapter on chemical affinity is probably more relevant to the trend of the history of chemistry in the 1960's.

The author's interest in the earlier period of chemistry together with his resolve to take the subject up to the chemical 'revolution' associated with the name of Lavoisier, leads to a sketchy treatment of the eighteenth century, which is the vital period in the development of chemistry as an exact science and this tends to throw the whole book out of balance. Although perhaps too reliant on secondary sources, this book makes a significant contribution to the history of science and it is particularly valuable at the present time when we are awaiting the publication of volume one of Professor Partington's monumental work.

M. P. Crosland

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

(*a*) Books Recieved for Review

Agassiz, L., *Essay on Classification*, Harvard University Press; Oxford University Press, London, 1962, pp. xxxiii + 268, 40s.

Alexander, P., *A Preface to the Logic of Science*, Sheed & Ward, London, 1963, pp. viii + 144, 15s.

Armour, L., *The Rational and the Real*, Martinus Nijhoff, The Hague, 1962, pp. vi + 97, D.fl. 8.25

Berg, J., *Bolzano's Logic*, Almqvist and Wiksell, Stockholm, 1962, pp. 214, Kr. 32

Bocheński, J. M., *Philosophy, an Introduction*, D. Reidel, Dordrecht-Holland, 1962, pp. 111, D.fl. 9.75

Brown, R., *Explanation in Social Science*, Routledge & Kegan Paul Ltd., 1963, viii + 198, 25s.

Butler, R. J., *Analytical Philosophy*, Blackwell, Oxford, 1963, pp. vi + 235, 25s.

A collection of near popular essays representing several strands of analytical philosophy. A symposium on effects and chapters on causation and counterfactuals and causal laws of immediate interest to philosophers of science.

Cardwell, D. S. L., *Steam Power in the Eighteenth Century*, Sheed & Ward, London, 1963, pp. ix + 98, 12s. 6d.

Cohen, M. and Nagel, E., *An Introduction to Logic*, Routledge & Kegan Paul, London, 1963, pp. ix + 226, 8s. 6d.

Colodny, R. G. (Ed.), *Frontiers of Science and Philosophy*, University of Pittsburgh Press, 1963, pp. 288, $7.50

Dantzig, T., *The Language of Science*, George Allen & Unwin Ltd., London, 1963, pp. ix + 340, 25s.

Davie, D., *The Language of Science and the Language of Literature*, Sheed & Ward, London, 1963, pp. 88, 10s. 6d.

Dopp, J., *Logiques construites par une méthode de déduction naturelle*, Universite de Louvain, 1962, pp. 191, Fr.B. 220

Drake, S. (foreword by A. Einstein), *Galileo, Dialogue Concerning the Two Chief World Systems*, University of California Press; Oxford University Press, London, 1962, pp. xxvii + 505, 24s.

Durkheim, E. and Mauss, M., *Primitive Classification*, Routledge & Kegan Paul, London, 1963, pp. xlviii + 96, 18s.

Edelen, D. G. B., *The Structure of Field Space*, California University Press; Cambridge University Press, London, 1963, pp. ix + 225, $10.50, 84s.

Gentile, G., *L'Esprit, La Vérité et L'Histoire*, Aubier, Paris, 1962, pp. 329

Grene, Marjorie, *A Portrait of Aristotle*, Faber & Faber, London, 1963, pp. 271, 30s.

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Hausdorff, F., *Set Theory*, 2nd edition, Chelsea Publishing Company, New York, 1963, pp. 352, $6.50

Hesse, Mary B., *Models and Analogies in Science*, Sheed & Ward, London, 1963, pp. 150, 15s.

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King, L. S., *The Growth of Medical Thought*, University of Chicago Press, 1963, pp. ix + 254, $5.50

Kuhn, T. S., *The Structure of Scientific Revolutions*, The University of Chicago Press, Chicago, 1963, pp. xv + 172, 22s. 6d.

Larson, D. B., *The Case Against the Nuclear Atom*, North Pacific Publishers, Oregon, 1963, pp. vii + 138, $4.50

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Michotte, A., *The Perception of Causality*, Methuen, London, 1963, pp. xxii + 418, 45s.

Miller, H., *Progress and Decline*, Ward Ritchie Press, California, 1963, pp. 287, $10.00

Moulton, F. R. and Justus J. Schifferes (Eds.) *The Autobiography of Science*, John Murray London, 1963, pp. xxxiii + 748, 30s.

'An anthology of the key passages of the master works of all sciences.' Arranged historically and by subject.

Muller, C. C., *Science and Faith*, Louvain, 1962, pp. 46, NF 35

Newman, J. R., *Science and Sensibility*, Simon & Schuster, New York; George Allen & Unwin, London, 1963, Vol. I, pp. x + 372, Vol II, pp. x + 309, 75s.

This is a miscellany of essays by the author on history of science, including essays on particular scientists and philosophers. The second volume contains a part in which causality and chance in modern physics, determinism, and reason and chance is scientific discovery is discussed.

Pap, A., *An Introduction to the Philosophy of Science*, Eyre & Spottiswoode, London, 1963, pp. xiii, + 444, 70s.

Popper, K. R., *Conjectures and Refutations*, Routledge & Kegan Paul, London, 1963, pp. xiii + 412, 55s.

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Ruytinx, J., *La Problématique philosophique de l'unité de la science*, Société d'Edition, Paris, 1962, pp. vii + 368, NF 22

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(*b*) ARTICLES

General

Dabska, I., 'Dwa Studia z teorii naukowego poznania' ('Two Essays on the Theory of Scientific Cognition') *Torun Sci. Soc.*, 1962, **12**

Ezorsky, G., 'Truth in Context' *J. Phil.*, 1963, **5**, 113-136

Grünbaum, A., 'Science and Man,' *Persp. in Biol. and Med.*, 1962, **4**, 483-502

Gumanski, L., 'Elementy sadu a istnienie' ('Elements of Judgment and Existence'), *Torun Sci. Soc.*, 1961, **11**

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Rose, K., 'Impossible Problems,' *New Philos.*, 1961, **64**, 257–266

Schaff, A., 'Language and Thought,' *Stud. Filoz.*, 1663, **1**, 3–43

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Broad, C. D., 'Hume's Doctrine of Space,' Dawes Hicks Lecture on Philosophy, British Academy, *Proc. Brit. Head.* 1961, **47**, 161–176

Chihara, C. S., 'Mathematical Discovery and Concept Formation,' *Philos. Rev.*, 1963, **72**, 17–35

Costa de Beauregard, O., 'Deux Problemes en epistemologie du temps de la physique,' *Revue Inter. de Philos.*, 1962, **61–62**, 358–378

Gale, R. M., 'Some Metaphysical Statements about Time,' *J. Phil.*, **9**, 1

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Poulet, G., 'Stendhal et le Temps,' *Revue Inter. Philos.*, 1962, **61–62**, 395–413

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Whitrow, G. J., 'On the Nature of Time,' *Revue Inter. de Philos.*, 1962, **61–62**, 319–333

Biological Sciences

Rensch, V. B., 'Probleme der Willensfreiheit in Biologischer und Philosophischer Sicht,' *Hippokrates*, 1962, **24**, 1–14

RECENT PUBLICATIONS

ANNOUNCEMENTS

MEETINGS OF THE BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

1962

8 October: Mr J. R. Lucas, 'The One Concept of Probability'
5 November: Mr D. E. Broadbent, 'Theories of Short-term Memory'
3 December: Professor J. H. Woodger, 'Lopsidedness, its Cause and Cure'

1963

14 January: Presidential Address: Professor R. B. Braithwaite, 'Believing (as opposed to Considering) Natural Laws'
11 February: Professor R. G. Lipsey, 'Problems of Scientific Discovery in Economics'
11 March: Dr D. W. Sciama, 'The Unity of the Universe'
29 April: Mr G. J. Warnock, 'Conceivability'
20 May: Professor S. Körner, 'Generalisation and Idealisation'
17 June: Professor R. Rudner, 'Nothing but the Truth'

BRITISH SOCIETY FOR THE PHILOSOPHY OF SCIENCE

Eighth Annual Conference: 20th-22nd September 1963

A conference will be held at St Hugh's College, Oxford, from Friday evening to Sunday afternoon, 20th-22nd September. The conference fee will be £3 15s. 6d. for full residence. Those staying shorter periods will be charged a separate registration fee of 10s. Single rooms are available at 30s. a day, bed and breakfast 21s. Non-residents' meals are coffee 1s., lunch 3s. 6d., tea 1s. 9d., dinner 5s.

The programme will be as follows:

Friday evening: *Models of Biological Growth Processes*
Chairman—Professor D. R. Newth
Speakers—Dr H. Kacser and Mr F. T. C. Harris
Saturday morning: *A Priori and Empirical in Economics*
Chairman—Professor G. H. L. Shackle
Speakers—Dr Ann Martin and Professor J. R. N. Stone
Saturday evening: *Value Judgment in Economics*
Chairman—Professor J. R. Hicks
Speakers—Professor R. L. Meek and Mr K. Klappholz
Sunday morning: *Mathematical Logic in Relation to the Philosophy of Science*
Chairman—Professor W. C. Kneale
Speakers—Dr G. T. Kneebone and Mr M. H. Lob

Those wishing to attend are asked to write to the Conference Secretary, Mr F. T. C. Harris, Department of Biology as Applied to Medicine, Middlesex Hospital Medical School, London W.1, not later than 9 September. Please enclose with your application a cheque made out to the British Society for the Philosophy of Science.

RECENT PUBLICATIONS

SCIENCE IN INDIA

THE Council of Scientific & Industrial Research has established a History of Science Unit to carry our primarily studies on the historical development of science in India. The Unit is charged with compiling a comprehensive bibliography of scientific manuscripts available in various Indian languages in different periods of Indian history, preparing subject and period histories of different branches of science, and a comprehensive history of the development of science and technology in this country. It will also work on the social implications of science and the impact of science on modern Indian society.

THE ARISTOTELIAN SOCIETY

PRIZE FOR AN ESSAY IN THE PHILOSOPHY OF MATHEMATICS

A donor who wishes to remain anonymous has given the Society £150 to be awarded as prize money in an open competition for essays upon a topic or topics in the Philosophy of Mathematics. An essay which is to be considered for an award may be either a philosophical investigation of some fundamental concept of mathematics, or an historical study of some notable contribution to the philosophy of mathematics, or a critical examination of the achievements and limitations of current methods of mathematical logic in dealing with some specific problem of the philosophy of mathematics. It should be typed in English, French, or German (2 copies) and it should not be a work that has already been published, or is at the time being considered for publication. It may be a dissertation which the candidate has previously submitted in a Ph.D. or similar examination. The Judges have discretion to divide the prize money as they think fit provided that they shall not divide it among more than three candidates and shall not give an award to any candidate unless in their opinion his work deserves such encouragement.

Entries should reach the Secretary, The Aristotelian Society, 30 Frith Street, London, W.1. by not later than 30 June 1964.

A. A. KASSMAN (Honorary Secretary & Editor)

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UNIVERSAL AND DIFFERENTIAL FORCES *

BRIAN ELLIS

IN his book *Space and Time* Reichenbach[1] makes a distinction between universal and differential forces. The distinction is roughly this. *Universal forces* are forces which 'act equally on' all things and which cannot be shielded against; *differential forces* are forces which either do not act equally on all things, or which can be shielded against. We shall have to say more concerning what is meant by 'acting equally' as the essay proceeds.

Reichenbach's purpose in making this distinction depends upon his thesis that universal forces of the kind he describes in the early chapters of his book (§§3, 4, 5, 6) can always be eliminated from physics merely by a change in our conception of what it means to say that something has remained the same in length (congruence definition). And since there is no *a priori* reason for preferring one congruence definition to any other, we should always prefer to change our congruence definition rather than admit the existence of such universal forces.

In this paper, I shall examine Reichenbach's initial distinction between universal and differential forces, and argue that the process of eliminating the former may not be so easy as he imagines. I shall show that although there is no *a priori* reason for choosing one congruence definition rather than another, it may not be a matter of indifference, and that a situation could arise in which it would be preferable, from a theoretical point of view, to adopt a congruence definition other than the one which he advocates, and thus admit the existence of universal forces of the kind he describes. For, as I shall show, we may adopt criteria which are logically independent of those which Reichenbach

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* Received 1.xii.62

[1] H. Reichenbach, *The Philosophy of Space and Time* (translated by Maria Reichenbach), Dover, 1958

admits for saying that something has remained the same in length. To prove this I shall show that the proposition that the universe and everything in it is expanding uniformly may have empirical consequences.

I *The Concept of Force*

There are many different concepts of force, and it is impossible in the space of a short article to discuss them all. Rather, I shall attempt to present a very general concept of force from which more particular concepts of force may be derived.

Let us first consider under what conditions we may say that a force is acting on a given system. There are two basic criteria. We say that a force is acting on a given system if and only if:

(1) The system persists in what we regard as an *unnatural*[1] *state*, or

(2) The system is changing in what we consider to be an *unnatural way*.

Corresponding to the first of these criteria we have *static forces*, and corresponding to the second, *dynamic forces*.

To illustrate these criteria, consider the following examples:

Example 1

In Aristotelian dynamics, it was considered natural for a body to return to its proper place by the shortest path. A freely falling body was, therefore, not considered to be subject to the action of any forces. A force was required only to *arrest* this natural motion. Again, in Medieval dynamics, the uniform motion of an object along a horizontal plane (a rolling wheel, for example) required the action of a force (impetus) for its maintenance. Any change of position of an object which did not result from its motion directly to its natural place was regarded as an unnatural change, and hence required a force for its explanation.

Example 2

In Cartesian and later in Newtonian dynamics, the Aristotelian-Medieval concepts of natural place and natural motion were replaced by new concepts. The concept of natural place was abandoned altogether, and uniform straight line motion was considered to be natural motion. A freely falling body must, therefore, be subject to the action of a force (gravity) and so, in general, must every body which does not move uniformly in a straight line, i.e. which undergoes a change of state of motion.

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[1] For an explanation of this term, see pp. 179–180.

Example 3

Every solid body which can be said to be *deformed* or in a state of *strain* must be conceived as having a natural shape and size. Otherwise these terms would have no meaning. The natural shape of a body is recognised by the following criteria. Is that shape which a body has when in circumstances in which:

(*a*) its shape is independent of its orientation, and

(*b*) the change in shape which the body undergoes when it is set in rotation about any axis is independent of the direction of its rotation about that axis.

Thus, a body sitting on a horizontal table is deformed, since under these circumstances, we believe that its shape is a function of its orientation. A body in free fall, on the other hand, which is not rotating about any axis fixed relative to the fixed stars, is to a high degree of approximation undeformed, since under these circumstances the conditions (*a*) and (*b*) are very nearly satisfied.

Every solid object, which by these criteria is deformed requires some force or forces to explain its deformation.

Example 4

Suppose that a body has a temperature greater than that of its surroundings. Such a body is conceived to be in an unnatural state. It must, therefore be subject to the action of a force (in the general sense in which I am using the term) which maintains it in that state. It must have some source of supply of *energy*. It is true that nowadays we seldom speak of forces in this connection, but we need not look far back into the history of science to find the word ' force ' so used. R. J. Mayer, for example, used it in this way, and so did Helmholtz in his famous paper ' On the Conservation of Force'. Indeed, until the mid-nineteenth century, the quantity we now call energy was always as described as a force. The fact that today we usually say that a force is acting on a system only if that system is deformed or accelerated need not, I think, prevent us from using the word ' force ' in a more general sense.

In general, then, I want to say that a force is acting on a given system if and only if the system persists in an unnatural state, or is changing in an unnatural way. But now we may ask: 'What do we mean by saying that a given shape or state of motion is natural or unnatural? My answer to this is very simple. We regard an object as being in a natural state if and only if we do not consider any causal explanation necessary for its persistence in that state. And by a causal explanation

I mean any explanation which invokes the existence of perturbing agencies. We consider an object to be in an unnatural state if and only if we consider that its persistence in that state requires some causal explanation.

We see, then, that our concept of force is tied to our concept of a natural state. Change this latter concept, as well we might, and we shall, at the same time, change our views about the forces which operate in nature.

Consider once again the transition from Medieval to Classical dynamics. Before the Cartesian view of natural straight line motion had been accepted, astronomers puzzled as to the nature of the forces which *compelled* the planets to move forward in their orbits around the sun. After Descartes, they asked a different question. They now wanted to know what *constrained* the planets to accelerate towards the sun rather than continue in a state of uniform straight line motion.

2 *The Concepts of Shape and Size*

We have seen in Section 1 that a force may be said to be operating on a given object if that object exists in an unnatural state. In particular, if an object is deformed in shape, then it is deformed by the action of some force. I have already given criteria for saying that an object is deformed. But in order to be able to use these criteria, it is obvious that we must have some independent criteria for saying that a given object *remains the same* in size and shape.

As a first step towards providing such criteria, we should notice that the size and shape of any given object is completely determined if we know the distances between every pair of points on its surface. In practice, of course, it is impossible to provide such complete specifications. Nevertheless, we can specify the size and shape of any given object as completely as we wish by this method. The problem of providing criteria for saying that a given object remains the same in size and shape therefore reduces to that of giving criteria for saying that the distance between any two points remains the same.

As a first attempt, we may suppose that the distance between two points remains the same if and only if the interval between those two points would always be spanned by the same rigid rod. But, unfortunately, this criterion is circular. For what is a rigid rod, but a rod which always remains the same in size and shape?

There are at least two ways by which we may successfully get out of this circle. Suppose that AB and BD are any two rods which are always found to be equal in length, i.e. which can always be placed together side by side without overlap. Let AC and CE be another pair of rods which are always found to be equal in length by this same criterion. Finally, let BC and DE be a pair of rods such that BC can always be stepped off twice along DE without overlap. Now, let us suppose that we try to arrange those rods according to the design of Fig. 1, and that this proves to be impossible. For example, let us suppose that the rod DE is always found to be too long.

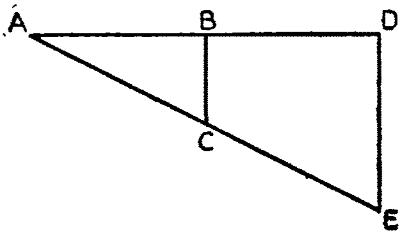

FIG. 1

What can we say in such a case? There are two fairly clear alternatives:

(1) We can say that the rod DE expands relative to the rod BC when it is moved into position—thus assuming that if the rods were rigid it would be possible to arrange them in the Euclidean pattern of Fig. 1.

(2) We can say that neither BC nor DE changes in length, and simply deny that the Euclidean theorem that if $AB = BD$ and $AC = CE$, then $DE = 2BC$ correctly describes a relationship between rigid rods. In other words, we can simply deny that space is Euclidean, and adopt some Non-Euclidean geometry, e.g. Riemannian geometry, in which $DE < 2BC$ appears as a theorem.

Corresponding to the first of these alternatives, we have the first way out of the circle. Rigid rods may be defined as rods which bear stable length relationships to each other, and which can always be arranged in Euclidean patterns. The concept of a rigid rod is thus defined *geometrically*. (There is, of course, no reason why the concept of rigidity should be tied specifically to *Euclidean* geometry. We could equally well say that rigid rods are rods which bear stable length relationships to each other, and can always be arranged in, say, *Lobachewskian* patterns. This is simply a matter of choice.)

The second way out of the circle depends upon taking the second alternative. We simply make it a matter of *definition* that rods which *always* under *all circumstances* bear stable length relationships to each other are rigid rods. And if it is found that such a set of rods cannot be arranged in the Euclidean pattern of Fig. 1, this just demonstrates that space is Non-Euclidean.

Neither definition is entirely satisfactory. For, as Grünbaum[1] has shown, it is possible that we may have two sets of rods (A) and (B), each of which are sets of rigid rods according to the geometrical criterion, although the various members of the set (A) do not bear stable length relationships to those of the set (B). And the second definition is obviously unsatisfactory for the same reason. At best, therefore, we can only say that the concept of rigidity is *circumscribed* by these definitions. But this point will not effect the main substance of my argument.

This second definition was the one adopted by Reichenbach. He defined a rigid body as a body which is not affected by (or which is insulated against) the action of *differential forces*—differential forces being ones which either do not act on all bodies equally, or which can be shielded against. To understand this definition, however, and to see why Reichenbach chose to define the notion of a rigid body in this way rather than geometrically, we must examine more closely his distinction between universal and differential forces.

3 *Reichenbach's Universal and Differential Forces*

Let us first consider the general concept of force used by Reichenbach when discussing the distinction between universal and differential forces. According to Reichenbach, a force is anything 'which is responsible for a *geometrical change*' (Reichenbach's italics). That is, a force is anything which is responsible for a change of *shape or size*. (What else is to be understood by a 'geometrical change'?) It is clear, then, that Reichenbach's concept of force is here much less general than mine. For I wish to say, roughly, that a force is anything which is responsible for a change in *any* respect that is thought to require a causal explanation. For example, if things change in electrical resistance, in mass, in specific heat, in refractive index, or in elasticity, then according to my criteria, forces must be active to produce these changes.

However, accepting Reichenbach's criterion for the existence of forces, we are immediately able to see what appears to be the basis of his distinction between universal and differential forces. For we can at once distinguish between two different sorts of criteria for saying that changes of shape or size have occurred, and thus we can classify forces according to the sorts of observations by which they are known to exist.

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[1] A. Grünbaum, 'Geometry, Chronometry and Empiricism', *Minnesota Studies in the Philosophy of Science* (Ed. H. Feigl *and* G. Maxwell), Vol. III (Minneapolis), 1962, p. 447

The distinction rests on the difference between observing changes of relative length *directly*, using the standard stepping off procedures of ordinary length measurement, and observing these changes *indirectly*, using geometrical criteria for invariance of relative length. Any force which, under certain circumstances, produces *directly* observable changes of relative length is a *differential force*, and any force whose existence is only known through the failure of certain geometrical relationships to hold is a *universal force*.

Thus, any force which can be insulated against is necessarily a differential force. For this follows from the concept of insulation. We say that a given object is insulated against the action of a force if and only if in the circumstances in which it is placed, it does not exhibit the changes (or deformations) which other objects in its vicinity exhibit. So that if a force is conceived to be something which produces a change of shape or size, it follows that an object which is insulated against the action of a given force must change in shape or size relative to other objects in its vicinity. In other words, these changes must be directly observable. Again, any force which affects different substances differently is necessarily a differential force. For let us suppose that two rods A and B of different materials are initially observed directly by the standard length comparison method to be equal in length. Then, if these rods are placed under the influence of a force which affects them differently, it must be possible to observe directly a change in their relative length.

The distinction thus presented seems reasonably clear. Heat is obviously a differential force. The existence of a field of heat can be detected by a direct comparison of the lengths of rods of different materials, or of the same material under different conditions of insulation. On the other hand, the force which would be required to explain the change in length of the rod DE relative to the rod BC (see Fig. 1) when it is moved into position would be a universal force if no relative length changes were directly observable whatever the material of the rods or the manner of their insulation. For it would seem that the existence of this change would then be known only through the failure of the Euclidean theorem to hold.

However, when we examine the use which Reichenbach later makes of his distinction between universal and differential forces, the matter becomes far less clear. For example, we are told that *gravitation* is a universal force. But while it is true that we cannot insulate a body against the effects of gravitation, it is not clear that gravitation *acts equally* on all things, since in the first place, the magnitude of a gravitational force is supposed to depend upon the mass of the body on which it acts. And in the second place, bodies can be distorted by gravitational forces, and this distortion is a differential effect, since it depends upon the elasticity of the material.

To explain this apparent lapse on Reichenbach's part, Grünbaum[1] draws a distinction between universal forces in the *literal* sense, and universal forces in the *metaphorical* sense, and suggests that when Reichenbach spoke of forces which were responsible only for geometrical changes, he spoke metaphorically. But, be that as it may, if we widen our concept of force in the way which I have indicated, it is not difficult to show that gravitational forces can also be eliminated from physics by the same kind of procedure as that by which universal forces in the metaphorical sense can be.

Universal forces of the kind which Reichenbach spoke initially can be eliminated from physics by a change in our concept of congruence, and hence by a change in our concept of what it is to say that an object remains the same in size and shape, and hence by a change in our concept of natural size and shape. Gravitational forces, as I shall show, can be eliminated from Newtonian physics by a change in our concept of natural motion.

4 *The Elimination of Universal Forces*

First, let us examine more carefully what is involved in the elimination of universal expansive forces. Consider the following simple example given by Reichenbach. Suppose that we have a circular piece of wire on which is attached a pointer which spans a diameter of the circle (Fig. 2). Suppose that both the wire and the pointer are rigid according to Reichenbach's definition of rigidity. Suppose, further, that as this instrument is moved about, the pointer needle is observed to shrink relative to the diameter of the circle.

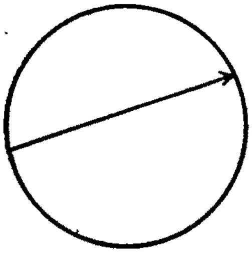

FIG. 2

Then, says Reichenbach, there are two things we can say.

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[1] op. cit. pp. 441 et seq.

(1) We can say that some universal force is operating which changes the relative lengths of the pointer and the piece of wire.

(2) We can say that space is curved, e.g. ellipsoidal, and that the Euclidean theorem that the ratio of the circumference to the diameter of a circle is constant does not apply to rigid bodies.[1]

If we adopt the second description, then clearly we shall not think that any changes in length have occurred, and hence none which require causal explanation. The need for a causal explanation has thus been eliminated by a change in our concept of what constitutes a change. If we hold that space is Euclidean, then we hold certain geometrical criteria for saying that the length of a given object remains the same. To adopt Reichenbach's procedure, however, we must abandon these geometrical criteria and adopt his own non-geometrical ones, according to which it is simply false to say that any changes of length have occurred.

5 *The Elimination of Gravitational Forces*

The proof that gravitational forces may be banished by a change in our conception of what constitutes a change of motion is not very difficult. Let us first recall that according to Newton's Law of Gravitation any two bodies A and B of masses m_A and m_B respectively attract each other with a force f given by the formula:

$$f = G\frac{m_A\, m_B}{r^2} \qquad (1)$$

where r is the distance between their centres of mass. And, provided that no other forces act on A and B, the absolute acceleration of A towards B, a_A, is given by the formula:

$$f = m_A\, a_A \qquad (2)$$

Hence, from (1) and (2)

$$a_A = G\frac{m_B}{r^2} \qquad (3)$$

Similarly

$$a_B = G\frac{m_A}{r^2} \qquad (4)$$

---

[1] It seems to me, incidentally, that the matter is not as simple as Reichenbach makes out since the existence of a supposed universal expansive force can in principle be detected other than geometrically. In consequence, the elimination of a universal force by a change of geometry may only confront us with the new problem of explaining these other non-geometrical effects. I shall elaborate on this point in section 6.

And, from (3) and (4) it follows that the *relative* acceleration a_{AB} of A and B towards each other is given by the formula:

$$a_{AB} = G\frac{m_A + m_B}{r^2} \tag{5}$$

It is therefore a consequence of Newtonian mechanics that every body accelerates towards every other body in the universe with a relative acceleration directly proportional to the *sum* of their masses and inversely proportional to the square of the distance between their centres of mass unless they are acted upon by some non-gravitational forces.

The method by which gravitational forces may now be eliminated should be clear. We need only change our conception of what constitutes natural motion. Instead of supposing, as Newton did, that natural motion is uniform straight line motion, and adopting the convention that a body continues in its state of rest or uniform motion in a straight line unless it is acted upon by a force, we may take as our first axiom of motion that *every body accelerates towards every other body in the universe with a relative acceleration directly proportional to the sum of their masses and inversely proportional to the square of the distance between them, unless it is acted upon by a force.*

To see that this axiom achieves the desired result, let us follow out some of its consequences. In the first place, it follows from this axiom that if we have a finite number of objects of finite mass sufficiently far removed fom each other, then their relative accelerations will tend to zero. And, assuming our galaxy to be such a system, it follows that any body sufficiently far removed from other bodies in the galaxy will continue in its state of rest or uniform motion in a straight line relative to the various stars in the galaxy, unless it is acted upon by a force. The 'fixed stars' may therefore be taken as a frame of reference for the determination of 'absolute' accelerations in Newtonian Mechanics.

Again, assuming that the galaxy is a system of objects sufficiently far removed from each other to constitute what I shall call an *absolute* frame of reference, it follows that the centre of mass of the solar system must continue in its state of rest or uniform motion in a straight line relative to that frame of reference. Taken individually, of course, the planets must accelerate towards each other, towards the sun and the frame of reference of the fixed stars. But this acceleration is a natural acceleration according to our conception. No force is required to explain it.

Confining ourselves now to the earth, the parabolic motion of a projectile (as modified by Coriolis' deflection) must now be regarded as natural motion. A body which is accelerating towards the centre of the earth with an acceleration g is not subject to the action of a force. A force is only required to arrest that acceleration, e.g. to impart to it an acceleration equal and opposite to its natural acceleration. The measure of that force is the weight of the body.[1]

The new conception of natural motion, and hence of what constitutes a *change* of state of motion, represents one further step in the direction which Descartes (and subsequently Newton) took in advance of Aristotle. In Aristotelian mechanics, any change of position, apart from a change which brings a body back to its natural place by the shortest path, required some force for its explanation. A change of position was regarded as a change of state. With Descartes, mere changes of position resulting from uniform translatory motion were not regarded as changes of state. Only changes of velocity were so regarded. In this new conception, which I am now putting forward, mere changes of velocity in accordance with the formula (5) require no explanation. They do not represent changes of state. Only changes of velocity which are not in accordance with this formula are regarded as changes of state of motion.

We see then that gravitational forces can be eliminated from physics by the same kind of technique as that by which Reichenbach proposed to eliminate any supposed universal expansive forces. And this distinction between the metaphorical and the literal senses of 'universal force' seems not to be an important one in this context.

6 *The General Distinction between Universal and Differential Forces*

It is tempting to conclude from this that universal forces should be defined simply as forces which can be eliminated from physics merely by a change in our conception of change. But we must not draw this conclusion too hastily. For it is not difficult to show that all fields of force can, in principle, be eliminated from physics in this way.

According to my conception, a force can be said to be acting on a given system if and only if the system exists in an unnatural state (or is changing in an unnatural way). The existence of a force thus *entails and is entailed by the effect it is supposed to produce.* Forces are therefore very queer scientific entities. They are not like other theoretical entities, such as atoms or genes, since the existence of atoms or genes is not entailed by the existence of the effects they are supposed to produce. Moreover, forces are radically different from ordinary causes since causes and effects are always *logically independent existences.* It need not surprise us, therefore, if we should find that forces are logically superfluous entities in science.

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[1] Note that this implies that there is now no distinction to be drawn between gravitational and inertial mass.

Let us first make a distinction between two different kinds of force laws—*laws of action of force* and *laws of distribution of force.* A law of action of force enables us to fix the magnitude of the force acting on any given system *from a knowledge of the magnitude of the effects* produced in that system. It may or may not be an empirical law. The following are examples of laws of action of force.

(1) Newton's Second Law of Motion.
(2) Hooke's Law.
(3) The Law of Torsion.
(4) The law relating shear stress and shear strain.
(5) $\Delta T = C\,M\,Q$ (where ΔT is the increase in the absolute temperature of a body, C is its specific heat, M is its mass, and Q is the heat supplied).

A law of distribution of force, on the other hand, enables us to predict the magnitude of the force acting on any given system from a knowledge of its characteristics *other than* those which are affected by the force, and of the spatial relationships of the system to *other* systems in its vicinity whose characteristics we also know. The following are examples of laws of distribution of force.

(6) The Law of Gravitation.
(7) The Law of Electrostatic Attraction.

Now a law of distribution of force can always be combined with a law of action of force to give us *a law of distribution of effects.* We have already seen how this can be done in the case of gravitation. And since the magnitude and existence of any force is only known from the magnitude and existence of its effects,[1] it follows that the whole empirical content of a law of distribution of force is contained in the corresponding law of distribution of effects. Consequently, since it is always open to us to regard any changes (or states) as natural, i.e. as changes (or states) which do not require causal explanation, then we may always consider any changes which occur according to a given law of distribution of effects as natural changes, and attempt only to explain any observed deviations from these supposed natural changes.

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[1] Of course, we can argue to the existence of effects inductively, and hence to the existence of forces inductively.

The distinction between universal and differential forces cannot therefore be made according to the criterion of eliminability unless we are prepared to count all field forces as universal forces. The distinction, however, can be made by considering the nature of the various laws of distribution of effects. We may say that a *universal field force is one whose law of distribution of effects contains only universal constants. A differential field force is one whose law of distribution of effects contains system-dependent constants.*

To make this distinction clear, of course, I must say what is meant by *universal* and *system-dependent* constants. I shall do this very briefly. A universal constant is any *constant* which appears in the expression of a scientific law which is always and everywhere the same. A system-dependent constant is any *parameter* which appears in the expression of a scientific law whose value depends upon the particular system to which the law is applied.

7 *Further Remarks concerning the Elimination of Universal Forces*

Finally, I wish to examine a certain presupposition of Reichenbach's work on the concept of rigidity. It is that changes in length are only detectable

(*a*) by the ordinary stepping off procedures of fundamental length measurement, or

(*b*) geometrically.

It seems to me that there are other logically independent criteria for saying that the length of any given object has changed or remained the same, and that, consequently, universal expansive forces may not be so easily eliminated from physics as Reichenbach imagined.

To prove this, I shall consider the hypothesis that the universe and everything in it is expanding uniformly, and demonstrate that this hypothesis may have *empirical consequences.* If my proof is valid, then it *must* be possible to give independent criteria for invariance of length, for it is obvious that such a change would not be detectable by either of the methods (*a*) or (*b*).

Let us assume that we have the following instruments at our disposal:

(1) A spring balance, calibrated to enable us to measure the weight of any given object directly in dynes.

(2) A set of standard masses and a beam balance with which we can measure the mass of any given object in grams.

(3) Two kinds of clocks—a *gravitational clock* and a *light clock*. An ordinary pendulum clock will do as an example of a gravitational clock. (I have called it a gravitational clock since its functioning depends upon the existence of a gravitational field). A light clock is any clock constructed according to the assumption that light travels equal distances in equal times. Let us suppose that the light clock and the gravitational clock are synchronised.

(4) A set of measuring rods.

Let us also assume that overnight the universe and all it contains doubles in size. (I shall discuss the Positivist objection that this assumption is meaningless below. See pp. 192–3). And, let us further assume that the laws of nature remain the same, i.e. that the behaviour of clocks and other instruments in the expanded universe is predictable from a knowledge of the laws governing their behaviour in the original one. In other words, I assume that the behaviour of things in the expanded universe might be predicted by the sort of procedure we might use to predict what would happen if, say, the *earth* and everything on it were to double in size.

We have as our starting point the proposition that the value of g, the gravitational acceleration is calculable from the results of two logically independent sets of measurements. Firstly, it may be determined for any point in space by the relationship $g = \frac{W}{M}$, where W is the weight of any object measured on our spring balance, and M is its mass, measured on our beam balance. Secondly, it may be determined by the ratio $\frac{2s}{t^2}$, where s is the distance measured in centimetres and t is the time taken for any given object to fall that distance measured in seconds.

We also have that mass is measurable independently of space and time. If the beam balance doubles in size, its symmetry is still preserved. Hence the measure of the mass of any given object in the expanded universe must be the same as in the original. Further, the weight of any given object at any given place as determined on our spring balance must be the same in the expanded universe as in the original. Otherwise, we have an immediate observational consequence. Therefore, the measure of g, as determined by the ratio $\frac{W}{M}$ must remain the same in the expanded universe as in the original if there are to be no observational consequences of the expansion.

But g is also determinable by the ratio $\frac{2s}{t^2}$. Consequently, if the value of g as calculated by this ratio is to remain the same as that calculated by the ratio $\frac{W}{M}$ in the expanded universe, then our clocks must slow down by a factor of $\sqrt{2}$. For if s becomes $s^1 = 2s$, then t^2 must become $t^{12} = 2t^2$ if there are to be no observational consequences, i.e. $t^1 = \sqrt{2}t$.

Assuming that the value of g remains the same in the expanded universe, such a slowing down of gravitational clocks is indeed predictable. For the period of a pendulum is given by the formula :

$$T = 2\Pi\sqrt{\frac{L}{g}}.$$

And, assuming that L becomes $L^1 = 2L$, and that g becomes $g^1 = g$, then T must become $T^1 = \sqrt{2}T$.

However, the case is different with light clocks. For, assuming that there is no observable change in the measured velocity of light in the expanded universe, i.e. assuming that c becomes $c^1 = c$, then, when s becomes $s^1 = 2s$, t must become $t^1 = 2t$. In other words, our light clocks and gravitational clocks must both slow down, but at different rates. Relative to our gravitational clocks, our light clocks must appear to be slow by a factor of $\sqrt{2}$. It follows then that there is at least one observable consequence of the supposition that the universe and everything in it is expanding uniformly. And, we may express this in the form of a dilemma.

Either:

(1) The weight of any given object will appear to change in the expanded universe, and in that case the calculated value of g (the ratio $\frac{W}{M}$) will also appear to change, or

(2) The weight will not appear to change, and hence the calculated ratio $\frac{W}{M}$ will be the same in the expanded universe but, in that case, either the calculated value of g from the ratio $\frac{2s}{t^2}$ will not equal g as calculated from $\frac{W}{M}$, or the velocity of light as calculated from the ratio $\frac{s}{t}$ will appear to increase.

It should also be pointed out that what I have offered is by no means a *proof* that there would be observational consequences if the universe and everything in it were to double in size. It is, of course, *logically* possible that there should be no such consequences. From the fact that two different kinds of clocks would appear to slow down at different rates *relative* to a clock which is not affected by the expansion, it does *not* follow that observers in the expanded universe, armed with gravitational and light clocks, would observe *each other's* clocks to be slow and fast respectively. We cannot say how they would look to each other. Nevertheless, the argument does show that *if* clocks were to behave in the way predicted, the hypothesis of the expanding universe would be a good explanatory hypothesis. And that is all that we can ever demand of any theory.

Moreover, since the only observations which we need to make in order to check the hypothesis of the expanding universe are such that they can be made in any confined region of space where there is a gravitational field (e.g. on the surface of the earth), it follows that if the universe were to expand differentially, i.e. so that rods which are rigid according to Reichenbach's criteria could no longer be fitted into Euclidean patterns, then this hypothesis would be checkable merely by observing the behaviour of different sorts of clocks. Hence, the hypothesis of the differentially expanding universe would not only do the job it was designed to do, it would also be independently confirmable.

Of course, Reichenbach's procedure is still open to him. He can still maintain his definition of a rigid body since this is conventional and determine the geometry of space by means of length measurements based on this definition. But, supposing that the predicted effects actually occur, he is now faced with the problem of explaining the diverse behaviour of different sorts of clocks in different parts of space. He will have to suppose that the velocity of a light ray is a function of the curvature of the space through which it passes. And to explain this, it seems, he will have to suppose the existence of some universal force which affects all light rays equally—a supposition of precisely the kind he would like to avoid. Of the two hypotheses, therefore, that of the differentially expanding universe would, in these circumstances, seem to be preferable.

Before leaving this argument, there is one objection which I must counter. It may be objected that since my initial assumption, viz. that the universe and everything in it doubles in size, is empirically meaningless, no conclusions whatever can be drawn from it. To this I would reply by drawing a distinction between different kinds of understanding. Suppose I say that *n* is expanding. Then for most values of *n*, certain observational consequences are clearly understood. But, of course, we may always seek, and sometimes find, further observational consequences of expansion, and then explain these further consequences. The discovery of structural weakening is a case in point. Now, when in place of ' *n* ', I write ' The universe and everything it it ', no observational consequences are clearly understood. But we must not draw the conclusion too hastily that nothing is being asserted. For there is nothing to prevent us from discovering some further consequences of expansion in general which may serve as a criterion for the truth or falsity of this assertion. I want to say that we understand the assertion in question *analogically*. We understand it as being an assertion of the form ' *n* is expanding uniformly '. And, as such, we may search amongst the more peripheral criteria for saying that something is expanding to try to discover some observational consequences.

Now, it is only analogical understanding which is required for the above demonstration. In place of the universe we may think of any finite region of the universe which has a suitable supply of instruments. The demonstration then shows that in any such region, there would be observational consequences even if it were not physically possible for us to see beyond this region. And we have thus discovered observational consequences which would give empirical significance to the assertion made about the universe as a whole.

8 *Remaining the Same in Length*

It follows from the argument presented in section 7 that it must be possible to give criteria for saying that a given object remains the same in length independent of

(*a*) direct length comparisons, and
(*b*) geometrical considerations.

I must now try to state these criteria explicitly.

First, let me introduce a technical term. I shall say that any quantitative law in which length (or distance) appears as a variable is a *length law*. Thus, Hooke's Law, Newton's Law of Gravitation, Kepler's Third Law of Planetary Motion, Galileo's Law of Free Fall, the Law of Inertia and the Principle of the Constancy of the Velocity of Light are all examples of length laws. In each case, length (or distance) appears as one of the variables.

In the mathematical expressions of these length laws, various system-dependent and universal constants appear. Now we have seen that the values of some, at least, of these constants should be observed to change if the universe and everything in it doubled in size. And it is this fact which gives us our independent criterion for invariance of length. We may say that the various members of a given set of rods remain the same in length if and only if

(1) they retain the same length relationships to *each other* as judged by:
   (*a*) the method of direct length comparisons, and
   (*b*) some agreed geometrical criteria, and
(2) using these rods as standards of length, the same values of the various system-dependent and universal constants which appear in our length laws are obtained.

The application of these criteria has already been sufficiently illustrated.

I conclude, therefore, that Reichenbach was wrong in supposing that there are only two sorts of criteria which we can use for invariance of length. There is in fact a third criterion, viz. the invariance of the various constants which appear in our length laws. And this criterion is logically independent of Reichenbach's.

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THE CASE FOR AN AETHER*

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1 *The Propagation of Light*

RECENT discussion[1] on the significance of Special Relativity has involved the merits and demerits of an 'aether' interpretation. In particular, the validity of introducing a second *ad hoc* assumption to maintain such a theory has been challenged. It may therefore be of interest to take stock of the evidence and consequences relevant to an aether hypothesis, and to show that, among other conclusions, the Fitzgerald contraction and time-dilatation assumptions are by no means independent and that both may in fact be consequential on a single more fundamental concept.

The whole point of an aether hypothesis is that it provides a substratum for the propagation of light, more generally of electromagnetic activity. Yet this point is so taken for granted that its consequences are rarely considered. We have used the word, aether, in deference to its historical connotation; in fact we are not interested in the mechanical aethers of the nineteenth century but only in its substratum connotation. Our aether hypothesis requires only the existence of a basic inertial system (whose space is the 'aether') relative to which electromagnetic propagation is isotropic with velocity c. The hypothesis has support from a number of directions.

Recent advances in astronomical observation[2] and cosmological theory[3] suggest that the nature of the universe is governed by 'the cosmological principle'—that is, that apart from local irregularities, the universe and the laws of nature would appear the same from the viewpoint of any of the mutually receding galaxies. We can imagine a set of mutually receding observers—'the fundamental observers'—who have such equivalent viewpoints. Now the law of light-propagation must preserve this equivalence, so Professor McCrea has proposed the hypothesis that a light-ray travels such that it passes

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* Received 25. vii. 62

[1] E.g. the contributions by Dingle, Grünbaum, and Keswani in recent editions of this *Journal*

[2] B. Lovell, *The Exploration of Space*, London, 1962

[3] H. Bondi, *Cosmology*, London, 1961

every fundamental observer in its path with the same velocity c. This implies that the nature of light propagation is determined by the overall structure of the universe and that the family of fundamental observers virtually delineates a universal substratum relative to which the velocity of light has an absolute significance.

The substratum is not uniform since the fundamental observers are themselves in the relative motion. However at any locality[1] there will exist a unique reference frame, with a fundamental observer at its origin, relative to which the propagation of light has the velocity c and is isotropic. The system associated with this reference frame will be referred to as the basic inertial system, I_S, for the locality.

The substratum hypothesis is supported from other directions also. It would explain the observed independence of light-velocity to the motion of its source, it would permit of a simple physical interpretation of the phenomenon of astronomical aberration, it would be consistent with Maxwell's assumptions underlying modern electromagnetic theory—that is, that electromagnetic propagation has an absolute basis.

In all important respects our universal substratum fulfils the rôle attributed to the artificial aether of the nineteenth century.

2 *A meaning for Time-Dilatation*

Given the existence of a basic inertial system, I_S, the uniform motion of matter, as well as of light, takes on an absolute significance. We will consider as a second assumption, for the time being independent of the first, that the absolute motion of matter in I_S with velocity u is associated with a single effect, a length-contraction of motion such that this length is proportional to $\sqrt{[1-u^2/c^2]}$. It follows[2] that a moving rod of stationary length l, inclined at an angle θ with the direction of motion, has its length contracted to l' where

$$l' = \frac{l\sqrt{[1-u^2/c^2]}}{\sqrt{[1-(u\sin\theta)^2/c^2]}} \qquad (1)$$

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[1] The region occupied by a typical galaxy of diameter 30 kpc can be considered as such a locality, since taking Hubble's constant as 100 Km/sec/M pc, the velocity of light within such a galaxy and relative to its centre would differ by only one part in 100,000.

[2] The mathematical derivations justifying the relationship (1) and other consequences of our assumptions have their own intrinsic interest. They form a complete system which has been presented in an article entitled 'Neo-Lorentzian Relativity' and submitted for publication elsewhere. Some of these derivations are by no means original but we are interested here in showing that an 'aether' context provides them with an unambiguous setting and interpretation.

This assumption also has an experimental basis. It is consistent with the null-results of the Michelson-Morley experiments and with the more recent observations[1] of an absolute time-dilatation effect associated with uniform motion. We shall see that the existence of this phenomenon is a necessary consequence of our assumptions; in fact it is the only consequence which is not manifested as a null-effect, so its experimental confirmation would provide the most positive evidence of the validity of these assumptions.

The measurement of time requires the employment of a clock, that is, of a mechanism which provides a repetitive phenomenon; the duration of the phenomenon can then be taken as the unit of time. Following Builder[2] we can adapt our rod, of stationary length l to form a simple yet typical clock. If the rod has a mirror at each end to reflect a beam of light to and fro along its length, the unit of time, $\hat{t}$, can be taken as the interval between successive light reflections on one of the mirrors. When such a clock is stationary in I_S, the unit of time, $\hat{t}_S$, is given by

$$\hat{t}_S = 2l/c.$$

However, when the clock is moving with velocity u, the velocity of light relative to the rod is no longer c. In fact the to and fro velocities will, in general be different and if the rod-clock is inclined at an angle θ to the direction of its absolute motion, these two relative velocities will be

$$\begin{aligned} c_1 &= \sqrt{[c^2 - u^2 \sin^2 \theta]} - u \cos \theta \\ c_2 &= \sqrt{[c^2 - u^2 \sin^2 \theta]} + u \cos \theta \end{aligned} \qquad (2)$$

Remembering also that the clock is contracted according to (1), the unit of time, $\hat{t}_M$, for the moving clock will be

$$\begin{aligned} \hat{t}_M &= \frac{l'}{c_1} + \frac{l'}{c_2} \\ &= \frac{2l}{c}(1 - u^2/c^2)^{-\frac{1}{2}} \\ &= \beta \hat{t}_S \end{aligned}$$

where $$\beta = (1 - u^2/c^2)^{-\frac{1}{2}}$$

Thus the unit of time for such a clock is extended as a result of its motion, but its operation is independent of its direction in space. It follows that an interval of time, Δt_S, as measured by a stationary clock (or by a pair of synchronous stationary clocks), will have a shorter measure, Δt_M, according to a similar moving clock; exactly we have

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[1] E.g. the apparent variation of meson life with velocity, the transverse Doppler effect, the behaviour of photons in relation to the Mössbauer effect.

[2] G. Builder, 'Ether and Relativity', *Aust. J. Phys.*, 1958, **11**, 279

$$\Delta t_M = \Delta t_S \sqrt{[1 - u^2/c^2]} \tag{3}$$

A corresponding time-dilatation effect can be considered as applying to all phenomena involving electromagnetic impulses; hence if we assume, like Builder,[1] that physical interactions have an electromagnetic basis, then all natural phenomena, including macrocosmic and biological, will manifest the effect.

It is seen that, in this context, time-dilatation is shorn of its mystery, on the background of a universal time. It is the measurement of time which is effected by motion, the effect resulting from the interplay of two phenomena—the Fitzgerald contraction and the anisotropy of light propagation relative to a moving body. This anisotropy, for all inertial systems except the basic one, and the time-dilatation effect are both direct consequences of our assumptions, and their association leads to a number of interesting results.

3 *The Motion Effects in the light of Einstein's Measurements*

The time-dilatation effect is responsible for the different time-scales employed within different inertial systems. However it is the anisotropy effect which produces the different standards of simultaneity.

The criterion of simultaneity is based on Einstein's definition of synchronism. This provides that the clocks of two relatively stationary observers, A and A', are synchronous if a light-ray from one reflects a clock-reading from the other in accordance with the assumption that the reflecting light-ray travels with the same velocity in both directions. For observers stationary in the basic system, I_S, this assumption is well-founded, so that for such observers the above concept of synchronism has an absolute significance unique to I_S. However if the observers A and A' are associated with an inertial system, I_A, which is moving with velocity, u, relative to I_S, then a light-ray travelling between them will have to and fro velocities, c_1 and c_2 (relative to I_A), as given by (2). It follows that if A and/or A' adjust their clocks to satisfy Einstein's synchronism definition, then these will appear non-synchronous to the extent of $(\beta\, du/c^2) \cos\theta$, according to observers stationary in I_S; where d and θ are the respective I_S measures of the space-interval separating A and A', and of the angle that AA' makes with the direction of the velocity u.

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[1] G. Builder, Colloquium at University of New South Wales, 1960.

The concept of simultaneity of events at different places depends on an appeal to clocks stationed at these places and considered synchronous. Hence observers associated with I_A will differ from their I_S counterparts in their judgments of simultaneity. Moreover the synchronism discrepancy, $(\beta du/c^2)\cos\theta$, depends on the absolute velocity of the inertial system constituting an observer's reference frame, so that any two observers in relative motion will differ in their estimations (according to Einstein) of simultaneity.

Einstein's definitions for estimating the location and time of a distant event also depend on the synchronism criterion[1] and so incorporate the same discrepancy. This effect combined with the time-dilatation factor provides the differing observations of an event by observers in relative motion, precisely as described by the Lorentz transformation.[2] The transformation and its associated composition of velocities formula represent the relationships between measurements, carried out according to Einstein's definitions, but made with respect to *different* inertial systems; hence, as Builder[3] contends, these relationships have no relevance to measurements made with respect to a single system. Seen in this light, the composition of velocities formula foregoes its appearance of contradicting the laws of Newton and of simple vector algebra. The latter still apply to measurements referred to a single reference frame, whether these be displacements, time-intervals, or velocities. The relativistic formulae relate the corresponding measurements made with respect to different reference frames.

4 *The Operation of Relativistic Phenomena*

The synchronism discrepancy and the time dilatation factors operate in opposite directions. For example consider two observers, A and B, respectively associated with inertial systems, I_A and I_B, and with absolute velocities, u_A and u_B where $u_B > u_A$. Let us assume for convenience that A and B have synchronised their similar clocks when originally spatially coincident. The time-dilatation effect is more pronounced for B's clock, but in A's estimation this differential effect appears diminished, for a light signal transmitted by A, to observe B, leaves him with velocity $c-u_A$ and returns to him with velocity $c+u_A$, hence it reaches B later than A estimates on the assumption of light isotropy in I_A. Thus in A's view B's clocks are lagging but running somewhat faster than is apparent to observers in I_S.

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[1] Alternatively, it might be said that it is again assumed that the out and return paths of a reflected light-signal are of equal duration.

[2] The derivation of the transformation in this way leads naturally to its four-dimensional expression without the need for any prior assumption regarding the co-ordinates normal to the relative motion.

[3] G. Builder, 'The Constancy of the Velocity of Light', *Aust. J. Phys.*, 1958, 11, 457

The observational discrepancy is larger and operates in the opposite direction ($\cos\theta$ being negative now) with respect to B's observations of A and in fact predominates over the time-dilatation difference, so that in B's estimation it is A's clock which appears to be running slow. It is this type of interaction between the two effects which produces the reciprocity expressed by the Lorentz transformation. This interaction has the effect of placing the observers of all inertial systems on the same observational footing and of concealing their absolute motion as well as its associated effects.

This applies also to the anisotropy consequence which is completely concealed to a moving observer by virtue of the effects on his measuring rods and clocks which he uses to measure the velocity of light. In consequence of (1), (2), and (3) he will find that light appears to have the same velocity, c, in all directions and in all circumstances. He will be justified in postulating that the measure of the velocity of light is the same for all inertial systems and is independent of the velocity of its source. And since the observational discrepancy effect is ' built into ' his synchronism procedure, he will find that his observations (according to Einstein) are consistent with the assumption that his inertial system is equivalent to a stationary one.

It is seen that an aether hypothesis is capable of giving rational significance to Einstein's principles; both of these emerge as intelligible consequences of this approach.

The mutual cancellation of the effects of absolute motion makes it difficult to test the hypothesis of an absolute space. However one effect may, in principle be observable, if it exists. It follows from the theory[1] that the time-dilatation effect is greater for a traveller making an out-and-return journey with respect to a given inertial system than for an observer at rest in that system; and, that the resulting ' time-difference ' is an absolute phenomenon observable equally from the viewpoint of any inertial system. Further, since this effect, unlike the Fitzgerald contraction, can leave a permanent mark, for example on clocks, the phenomenon provides a test of the existence of an absolute effect associated with uniform motion.

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[1] S. J. Prokhovnik, ' Neo-Lorentzian Relativity '

The problem of reconciling the reciprocity phenomenon with the inequality of time experiences between traveller and 'stay-at-home' constitutes the notorious 'clock-paradox' for which many and diverse resolutions have been proposed. In a context of a basic inertial system the paradox does not arise since the nature of the phenomena involved is clearly exposed.

The point is that the reciprocity applies only during the periods of uniform motion. During the traveller's turning period he changes his inertial system and hence his observational viewpoint. With the necessary change (whether slow or rapid) in his absolute velocity, the measurement discrepancy, associated with the observation of his home-scene, is diminished or even reversed; so that instead of observing his home-clocks retarded they now appear advanced as he commences the journey home. During the return-period the reciprocity phenomenon operates again but leaves the home-clocks still in advance at the end of the journey. Hence the traveller's findings on reaching home conform with his observations during the journey as well as with those of all other observers.

An analysis of a particular journey is appended at the end of this article. It is to be noted that the analysis involves only the concepts of special relativity.

5 *Physical Implications of the Anisotropy Effect*

The authoritative approach to Special Relativity is rather ambiguous about the nature of the observed effects described by the Lorentz Transformation. The substratum interpretation is unequivocal about the absolute nature of these effects but distinguishes between their existence and their observation in the sense defined by Einstein.

The confirmation of the absolute mass and energy effects predicted by relativistic mechanics is a further pointer to the validity of an absolute basis to the theory. These effects are mathematically deducible by reconciling relativistic kinematics (Lorentz transformation, etc.) with Newton's first and second laws of motion (Conservation and rate-of-change of momentum). Hence they should also ultimately depend on our assumptions and particularly on its anisotropy consequence in relation to the interaction of matter with electromagnetic and gravitational fields.

A manner in which this may occur has already been suggested by Bastin[1]. He proposes the generalisation

$$F=\frac{Gm_1m_2}{d^2}\sqrt{\left[\frac{1+v/c}{1-v/c}\right]} \tag{4}$$

of the usual gravitational law, to extend to the case when one body of rest-mass m_1 is stationary relative to a substratum and the second of rest-mass m_2 is approaching the first with velocity v.

Bastin shows that his law can be inferred from the assumption that the propagation of gravitational activity is isotropic with respect to the substratum and has the same velocity as light. The hypothesis that both these forms of energy are propagated in a common fashion is now generally conceded and is also supported by the striking similarity of the mathematical laws governing electromagnetic and gravitational fields. Further the assumption provides a means of comprehending the concept of 'action at a distance'. As Bastin observes, the interaction of two bodies requires some form of communication between them. Assuming that some form (or forms) of flux (a manifestation of energy) is continuously emitted from a given body, then the flux density diminishes with distance from the source precisely in accordance with the inverse-square law of the distance.[2] The law of force can then be considered as due to the interaction of a body and a field associated with the flux emitted by another body; or alternatively, to the interaction of the fields associated with the bodies concerned, thus modifying the geometry of the space in which the bodies are moving.

In the case considered by Bastin the effective flux density is increased in the ratio $(c+v)/c$ relative to the moving body, and this combined with other considerations of lesser magnitude leads to the modification as in the law (4). By applying this to a simple model of the universe, Bastin is able to deduce, among other results, the equivalence of gravitational and inertial mass, the mass-energy equivalence formula and other relationships of relativistic mechanics. His approach gives physical significance to these results in terms of the relation of a body to the rest of the universe.

It is instructive to employ Bastin's approach to the case when both bodies are moving with the same velocity, v, in a common straight line[1] relative to the substratum. As before the rest-masses will be denoted by m_1 and m_2, and the distance of separation according to a stationary observer by d. The effective gravitational flux density due to its neighbour is now modified by a factor of $(c+v)/c$ for one of the bodies and by $(c-v)/c$ for the other, so that the corresponding law for this case is given by

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[1] J. A. Bastin, 'An Extension of the Newtonian Law of Gravitation', *Proc. Camb. Phil. Soc.*, 1960, 56, 401

[2] The flux emitted at a given instant can be considered as spread over the surface of an expanding sphere.

$$\left.\begin{aligned} F &= \frac{Gm_1m_2}{d^2}(1-v^2/c^2) \\ &= \frac{Gm_1m_2}{(d/\sqrt{[1-v^2/c^2]})^2} = \frac{Gm_1m_2}{(d')^2} \end{aligned}\right\} \qquad (5)$$

Thus according to a stationary observer, the distance separating the two bodies is too short by a factor $\sqrt{[1-v^2/c^2]}$ to satisfy the usual law.

Assuming that electromagnetic fields are generated in the same manner, it follows that a similar argument will apply to the inverse-square laws for electrical and magnetic force. This suggests that the asymmetry of gravitational and electromagnetic activity relative to a moving system of particles would require a clustering of the particles in the direction of motion in order to maintain the equilibrium state of the forces within the system. It is seen from (5) that the required clustering would be exactly equivalent to a Fitzgerald contraction. This contraction would apply equally to a moving measuring rod, so that an observer, moving with the bodies m_1 and m_2, would find that the distance of separation had the measure

$$d/\sqrt{[1-v^2/c^2]} = d'$$

Hence for such an observer, the inverse-square law would apply in its usual Newtonian form—in accordance with the principle that absolute motion is not detectable and that the laws of nature take the same form with respect to all inertial systems.

The assumption that electromagnetic and gravitational propagation is isotropic is generally accepted but considered to have negative physical significance. However in terms of a basic inertial system the assumption must certainly have physical consequences for moving bodies as a result of the complex of anisotropy effects. Seen in this light the Fitzgerald contraction, as well as the derivative time-dilatation effect, emerge as necessary consequences of a single postulate, though the exact mechanism of the former effect may be more complex than suggested above.

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[1] Such a line can be considered as lying along the path, and its extension in either direction, of a light-ray, transmitted from one body to the other.

If this argument is valid then the resulting null-effects can no longer be considered fortuitous either. They are clearly inevitable and due to the mutual compensation of oppositely directed effects arising from a common cause—the absolute motion of a physical system. Hence in such a context the equivalence of inertial systems appears as a consequence of a generalised version of Newton's third law of motion, viz. 'The uniform motion of a physical system is associated with mutually compensating reactions which maintain the system in a state apparently equivalent to its stationary state.'

6 *The Significance of Einstein's Measurement Contribution*

The mathematics underlying the aether approach is neither difficult nor original. Much of it, is suggested in the arguments of Builder,[1] and some of the derivations have been employed—recently, for example by Frisch[2]—to illustrate the nature of relativistic phenomena. But without the background of a basic inertial system, such expositions are arguable and lacking in significance; the contraction and anisotropy effects are meaningless apart from the concept of absolute motion. On the other hand, in our context, these derivations fall into place as part of a complete and meaningful system.

It should be noted, however, that the substratum hypothesis, in itself, is insufficient for the derivation and interpretation of special relativity. This also requires the employment of Einstein's measurement definitions which give physical meaning to the symbols denoting the co-ordinates of a distant event. Only then is it possible to relate these symbols in terms of physical assumptions and hence to discern the consequences of these assumptions.

In his derivation of the Lorentz transformation, Einstein[3] applies, in fact, the concept of a 'stationary system' in which light propagation is isotropic and hence anisotropic with respect to his 'moving system'. However he sidesteps the implications of his derivation by appealing to his demonstration that the Lorentz transformation implies the universal constancy of the measure of light-velocity. Yet we have seen that the admission of anistropy does not preclude the identity of this measure with respect to all inertial systems, in fact it makes this universal constancy comprehensible.

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[1] G. Builder, loc. cit.

[2] O. R. Frisch, 'Time and Relativity', *Contemp. Phys.*, 1961, 3, 16. Frisch shows how the time-dilatation effect is manifested by a 'rod-clock' when its direction is either at right-angles or parallel to the direction of motion. He concludes that this phenomenon implies the existence of the length-contraction effect, but ignores the necessary co-existence of the anisotropy effect.

[3] A. Einstein, 'Zur Electrodynamik bewegter Körper', *Ann. der Phys.*, 1905, **17**, 891

Einstein's approach includes the admission of absolute effects associated with uniform motion within a closed path. The problem of reconciling this phenomenon with the denial of an absolute basis to uniform motion is usually overcome by attributing to 'space-time' the rôle of a fundamental sub-stratum[1] in which absolute effects are associated with non-geodesic paths ('world-lines'). This provides a mathematically consistent system but sidesteps the physical basis of these effects and so isolates the theory from the rest of physical science.

It is also possible to develop the theory in terms of a universal time for all observers and the absence of absolute effects.[2] This approach is applicable to a set of fundamental observers whose mutual recession is uniform and who can therefore be considered as sharing a common 'cosmic time'.

However no matter which approach is employed, Einstein's definitions play a key role. It was because of their absence that Lorentz and Poincaré were unable to link their aether assumptions with the Lorentz transformation and its consequences. Nor did it appear even to many subsequent aether proponents (e.g. Ives, Whittaker) that Einstein's contribution was of the utmost relevance to their approach.

The first serious analysis of Einstein's conventions in an aether context appears to be due to Builder.[3] Builder's argument is based mainly on physical and philosophical grounds; however his emphasis on the conventional nature of Einstein's measurements and his discussion of the anisotropy effect in relation to light-velocity measurements prepares the ground for a rigorous development of assumptions involving absolute motion in terms of a basic inertial system.

Builder considered that the concept of an absolute space provides the simplest hypothesis for the existence of absolute effects (associated with uniform motion) and for the known behaviour of light; that it has striking pedagogical and heuristic advantages and that it resolves the conceptual difficulties usually associated with the Special Theory. The mathematical amplification of Builder's approach bears out every one of these claims; further it associates Einstein's principles and his resultant theory with our present view of the universe and so renders them fully intelligible in this richer context.

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[1] This virtually constitutes a third assumption for the orthodox approach. It was introduced subsequent to Minkowski's re-interpretation of Einstein's theory in terms of four-dimensional geometry. Thus it is the orthodox version which is obliged to introduce an arbitrary (and extra-physical) assumption to 'save the phenomenon' —namely the existence of absolute effects which do not appear intelligible in the light of the initial (empirical) assumptions.

[2] This has been attempted by the author in a series of articles published in the *Journal and Proceedings* of the Royal Society of N.S.W., and also in more recent work not yet published.

[3] G. Builder, loc. cit.

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Appendix: Illustrative Analysis of an Out-and-Return Journey

Consider a journey in terms of our observers A and B of $\phi 4$, where $u_A = u$, $u_B = 2u$ during B's outward journey and $u_{B'} = 0$ during his return journey. Hence according to an observer S (stationary in I_S), the duration of each leg of the journey is the same and equal to T (say), and the maximum distance separating A and B is uT.

As usual we assume that the journey is sufficiently long so that the duration of the turning period can be considered negligible; and that the similar clocks of A and B were synchronised to read zero time at the instant of A and B's separation.

As B approaches the turning point, his clock-reading T_B is related to the corresponding I_S time T according to (2), viz

$$T_B = T\sqrt{[1-4u^2/c^2]}$$

A's corresponding clock-reading is similarly given by

$$T_A = T\sqrt{[1-u^2/c^2]}$$

Now a light-ray from A to B leaves the former with velocity $c-u$ and, after reflecting on B, returns to A with velocity $c+u$. Hence A's Einstein-measure, E_A, of the time of an event associated with B and his clock-reading T_B, differs from T_A by the synchronism discrepancy factor, $(\beta ud/c^2)\cos\theta$, where $d = uT$ and $\theta = 0$ for this case.

Thus

$$E_A = T\sqrt{[1-u^2/c^2]} - \frac{u^2T}{c^2\sqrt{[1-u^2/c^2]}}$$

$$= T_B/\sqrt{[1-v^2/c^2]} \qquad (6)$$

where $v = \dfrac{2u-u}{1-2u^2/c^2}$, v being the relative velocity of A and B as measured conventionally by either observer.

For B's observations of A the anisotropy discrepancy is in the opposite direction ($\theta = \pi$ for this case), so that B's corresponding measure, E_B, of an event associated with A and T_A is given by

$$E_B \doteq T\sqrt{[1-4u^2/c^2]} + \frac{2u^2T}{c^2\sqrt{[1-4u^2/c^2]}}$$
$$= T_A/\sqrt{[1-v^2/c^2]} \qquad (7)$$

(6) and (7) demonstrate how the reciprocity of observations operates during the period of uniform relative motion.

However the reciprocity is not operative during the turning period when B's velocity in I_S is diminishing. The anisotropy discrepancy affecting B's Einstein-observations diminishes accordingly so that during this period B observes A's clock apparently running faster than his own until uniform relative motion is resumed with B stationary in $I_{S'}$. B's estimate, E_B', is now related to T and T_A by

$$E_B' = T\sqrt{[1-4u^2/c^2]} = T_A\sqrt{\left[\frac{1-4u^2/c^2}{1-u^2/c^2}\right]}$$

For the remainder of the journey the reciprocity again operates. B observes the time-dilatation of A's clock relative to his own, and so during the return journey his clock regains $(T - T\sqrt{[1-u^2/c^2]})$ of the time lag expressed by the difference between $E_{B'}$ and T_A. A's reciprocal observation is now due to the diminution of the anisotropy effect as the distance of separation d contracts, the cancelling of this effect more than offsetting the time-dilatation factor. Thus the observations of both observers converge to their clock-readings at the instant of rejoining. These are

$$T_A = 2T\sqrt{[1-u^2/c^2]}$$
$$\text{and} \qquad T_B = T(\sqrt{[1-4u^2/c^2]}+1)$$
$$\text{so that } T_A - T_B = T(2\sqrt{[1-u^2/c^2]} - \sqrt{[1-4u^2/c^2]} - 1)$$
$$\doteq \frac{u^2}{c^2}T,$$

in accordance with the progressive observations of both A and B, and of any observer S stationary in I_S.

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THE SIGNIFICANCE OF INCOMPLETENESS THEOREMS *

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I WANT to start by considering certain fundamental differences between the major incompleteness theorems which have been discovered in researches in the foundations of mathematics during the past thirty years and the incomplete axiom systems which were found in the study of projective geometry during the last century. A confusion between these forms of incompleteness has led some mathematicians to underestimate the significance of the newer results and some philosophers to seek to understand the meaning of the new discoveries by reference to the technically simpler older work.

When it was discovered, for instance, that a system of projective geometry in two dimensions which postulated the axioms of incidence and Pappus' Theorem was incomplete because Desargues' Theorem on perspective triangles was not derivable in the system, then this incompleteness could be interpreted as a proof of the independence of Desargues' Theorem, postulated as a new axiom, from the other axioms. The impossibility of proving Desargues' Theorem is *surprising* in view of the fact that the corresponding theorem in three dimensions is derivable from the three-dimensional axioms of incidence, but it is nevertheless, I think, without philosophical significance because it throws no light on the nature of formal systems as such and imposes no limitations upon the axiomatic method.

The great modern incompleteness theorems which I shall consider are those due to Skolem and Gödel. Skolem's incompleteness theorem was discovered in an attempt to explain a paradox which Skolem himself found in the theory of sets.

The paradox out of which Skolem's incompleteness theorem arises, is produced by applying a result of Löwenheim's to a formalised set theory. Löwenheim showed that every consistent set of statements has a denumerable model, and so any formal system which admits some model (of the power of the continuum perhaps) has also a *denumerable* model. That is to say, for a consistent theory, we can find an interpretation in which all the objects, of which the theory

* Received 25.vii.62

treats, may be taken to be the natural numbers. Consider now some formalisation of set theory; according to the Löwenheim theorem we can find an interpretation of the membership relation of the theory in which all the *sets* of the theory are taken to be natural numbers. But in any adequate formalisation of set theory, using the familiar diagonal process, we can prove Cantor's theorem that the set of all subsets has a *greater* cardinal than the set itself:

Let S be a denumerable set and let a subset of S be denoted by a sequence of zeros and units, a zero in the nth place showing that the nth member of S is *not* in the subset, and a unit in the nth place showing that the nth member of S is a member of the subset. Suppose now that the set of all subsets of S *is* denumerable, and let it be enumerated as follows:

$$\begin{aligned} s_1 &= a^1_1, a^1_2, a^1_3, \ldots \\ s_2 &= a^2_1, a^2_2, a^2_3, \ldots \\ s_3 &= a^3_1, a^3_2, a^3_3, \ldots \end{aligned}$$

where each a^n_r is either 0 or 1.

Define $$b_n = a^n_n + 1 \pmod 2$$

and consider the subset

$$\sigma = b_1, b_2, b_3, \ldots$$

The subset σ differs from S_1 in respect of the first element of S since b_1 is 1 or 0 according as a^1_1 is 0 or 1, from S_2 in respect of the second element, and so on. Thus the subset σ does *not* occur in the enumeration $S_1, S_2, S_3, \ldots$ and the hypothesis that the set of all subsets of S is enumerable is disproved.

But by the Löwenheim theorem there is a model of set theory (supposed consistent) in which each set is associated with a natural number, so that in defiance of Cantor's theorem, the set of subsets *is denumerable*.

This is Skolem's paradox. The conclusion which Skolem himself drew from the paradox is that a formalisation of set theory can contain only *relatively non-denumerable* sets; i.e. sets which are non-denumerable only because the formalisation lacks the functions to enumerate them. In other words every formalisation of set theory must be incomplete in the sense that there are denumerable sets which cannot be *proved* denumerable within the theory. To justify this interpretation of the paradox one must observe that the proof of Cantor's theorem in some system starts by assuming that a certain mapping of a set on its subsets *exists* and derives a contradiction *in the system* from this assumption. Existence here of course means existence in the system, so that the conclusion to be drawn from the contradiction is, not that the mapping in question does not exist, but only that it does not exist *in the system*. Since Löwenheim's theorem assures us that the mapping does in fact exist, it follows that *there is a mapping which is not contained in the formal system,* so that the system is incomplete with respect to the class of mappings it contains. Unlike the situation in projective geometry we cannot remedy the deficiency by fortifying the system with another axiom; we could complete the system only at the price of rendering it inconsistent. Another way of expressing the result is to say that Löwenheim's theorem for any particular formalisation of set theory is not provable by means of the resources of that formalisation alone.

Of course the notion that there are no absolutely non-denumerable sets is not a new one. The sole ground we have for believing in the existence of a non-denumerable set lies in Cantor's theorem itself. But if we do *not* assume that the totality of subsets forms a set (and this is nothing *but* an assumption) then all that the diagonal process proves is that from *any* sequence of subsets we can construct another subset, just as from any natural number we can construct another, by adding one. And if we give up the axiom of subsets of course the Skolem paradox disappears.

There is nothing in the paradox itself to *force* us to give up the axiom of subsets. A constructivist who already rejects the set of all subsets on other grounds will not need to reckon with the Skolem paradox; and a mathematician seeking the greatest possible generality will have to remain content with *relative* non-denumerability. At best we can have a transfinite hierarchy of systems in each of which there are sets non-denumerable in a particular system but denumerable in a system of greater ordinal.

The Gödel Incompleteness Theorem

I come now to the major incompleteness theorem of mathematical logic, Gödel's Theorem that all sufficiently rich formal systems necessarily contain sentences which are neither provable nor refutable in the system. There are so many interesting facets to this result that I shall later consider the proof in some detail, but first I want to observe that in this theorem, as in the Skolem theorem for sets, the incompleteness revealed by the theorem cannot be filled by means of a new axiom; it is true that the particular undecidable sentence constructed in the proof can itself be postulated as an axiom, but the proof shows that the system so fortified will still contain undecidable sentences. The undecidable sentence in this Theorem of Gödel has the form $(\forall x)\ P(x)$; neither $(\forall x)\ P(x)$ itself, nor its negation $(Ex) \neg P(x)$ is provable in the system under consideration, system $\mathscr{F}$ say, but each instance of the universal sentence $P(x)$, namely $P(0), P(1), P(2), \ldots$ *is* provable in $\mathscr{F}$. Many attempts have been made to close the gap this theorem is thought to reveal in the proof structure of formal systems. The most obvious course to take would be to add to the proof resources of the system a new derivation scheme permitting the derivation of $(\forall x)\ P(x)$ from the infinite sequence of sentences

$$P(0), P(1), P(2), \ldots;$$

but this device entirely destroys the finite character of a proof process. In the Gödel Theorem as we shall see, the very possibility of *proving* an infinite number of sentences $P(0), P(1), \ldots$ without a prior proof of the universal sentence $P(x)$, was revealed for the first time. This aspect of Gödel construction has been cleverly exploited in a recent attempt to obtain a (relatively) closed proof system without introducing non-finitist proof schemata, and is in some respects one of the most interesting features of the Gödel Theorem.

As is well known, the heart of Gödel's construction is a one-to-one mapping of the syntax of a formalised arithmetic upon arithmetic itself. There are many ways known in which this mapping may be accomplished but I shall simply suppose that each primitive sign of some formal system of arithmetic $\mathscr{A}$ has been assigned a number, and that each sequence of primitive signs with numbers $n_0, n_1, n_2, \ldots, n_k$, whether forming a sentence or not, is given the number $2^{n_0} . 3^{n_1} \ldots . p_k^{nk}$, where p_k is the kth odd prime number. I shall further suppose that the formal system $\mathscr{A}$ contains all primitive recursive functions, either directly in the sense of admitting primitive recursive definitions as axioms or indirectly by having definition resources like existential and minimal operators. I may mention in passing that it is the failure to introduce this requirement and to show the fundamental part it plays, which vitiates most popular accounts of Gödel's work. I shall also assume that $\mathscr{A}$ is recursively axiomatisable, so that the predicate 'n is the number of an axiom' is expressed by a primitive recursive relation $A(n)$, i.e. is expressed within $\mathscr{A}$ itself by this relation. Such relations as 'n is the number of a one variable primitive recursive function', 'n is the number of a variable in formula number f', 'n is the number of a proof of formula f' and 'n is the number of the formula which results by substituting the numeral representation of number k in formula f' may all be shown to be primitive recursive. The key tools in establishing these results are the primitive recursiveness of the relation

$$(\mathrm{E}x)\{x < y \ \& \ \mathrm{R}(x, y, z)\}$$

where R itself is primitive recursive, and the reductions to primitive recursive definition of a related schema of definition known as definition by course-of-values recursion. To exhibit Gödel's undecidable sentence I denote by

$$St_f(\nu/r)$$

the number of the expression of the formal system $\mathscr{A}$ obtained by substituting the numeral representing the number r for the variable of number ν in the formula with number f, and by

$$\mathrm{Pr}(m, n)$$

the relation which says that m is the number of a proof of formula number n. Further let $\boldsymbol{\mu}$ be the number of the variable n and let $\mathbf{a}$ be the number of the sentence of A which we are denoting by

$$(\forall m) \neg \ \mathrm{Pr}(m, St_n(\nu/m)) \tag{i}$$

and finally let (Vm) G(m) denote the sentence obtained from (i) by substituting the numeral $\mathbf{a}$ for the variable n. Then neither $(\forall m)$ G(m) nor its negation is provable in $\mathscr{A}$. We observe first that since $(\forall m)$ G(m) is formed by substituting numeral $\mathbf{a}$ for the variable n in formula number $\mathbf{a}$, its number is therefore

$$St_{\mathbf{a}}(\nu/\mathbf{a}).$$

Hence if $(\forall m)$ G(m) were provable, and if $\mathbf{k}$ were the number of its proof, then

$$\mathrm{Pr}(\mathbf{k}, St_{\mathbf{a}}(\nu/\mathbf{a})) \tag{ii}$$

holds (and the formula in A which this represents is provable in $\mathscr{A}$, *because* Pr(m, n) is a primitive recursive relation, and this is one of the points where this fact is critical to the proof); but this contradicts the formula which $(\forall m)$ G(m) itself represents, i.e. that represented by

$$(\forall m) \neg \ \mathrm{Pr}(m, \mathrm{st}_{\mathbf{a}}(\nu/\mathbf{a}).$$

I emphasise again that the contradiction is in $\mathscr{A}$ itself, and so if $\mathscr{A}$ is consistent then $(\forall m)$ G(m) is not provable in $\mathscr{A}$. To prove that $\neg$ $(\forall m)$ G(m) is also unprovable I shall assume rather more than the consistency of $\mathscr{A}$, the so-called ω-consistency of $\mathscr{A}$, but this additional assumption could be dispensed with at the price of taking a rather more complicated sentence than $(\forall m)$ G(m). By the ω-consistency of $\mathscr{A}$ we mean that for any formula $\mathscr{G}(m)$ it is impossible to prove in $\mathscr{A}$ that

$(\mathrm{E}m) \neg\, \mathscr{G}(m)$ and $\mathscr{G}(0)$, $\mathscr{G}(1)$, $\mathscr{G}(2)$ . . . all hold simultaneously. First we observe that as a consequence of what we have already established, if $\mathscr{A}$ is simply consistent, then none of the numbers 0, 1, 2, . . . is the number of a proof of formula number $\mathrm{St}_{\mathbf{a}}(\nu/\mathbf{a})$, and so

$$\neg\, \mathrm{Pr}(m, \mathrm{St}_{\mathbf{a}}(\nu/\mathbf{a}))$$

is provable in $\mathscr{A}$ for any m, i.e. $\mathrm{G}(0)$, $\mathrm{G}(1)$, $\mathrm{G}(2)$, . . . are all provable. Hence by ω-consistency, $(\mathrm{E}m) \neg\, \mathrm{G}(m)$ is *not* provable. Thus we have seen that neither $(\forall m)$ $(\mathrm{G}m)$ nor its contrary is provable in $\mathscr{A}$, although each instance of the general formula, viz. $\mathrm{G}(0)$, $\mathrm{G}(1)$, $\mathrm{G}(2)$, . . . is provable. The formula $(\forall m)\ \mathrm{G}(m)$ is said to be undecidable.

I remarked earlier that Gödel's arithmetisation showed for the first time how it is possible in a formal system with finite proof procedure to prove all the formulae $\mathrm{G}(0)$, $\mathrm{G}(1)$, . . . *without first proving* $\mathrm{G}(m)$, (in fact even if $\mathrm{G}(m)$ is not provable); we have just seen an instance of this. In the general case, let $\mathrm{N}(k)$, be the number of the numeral representing k in A, and let h be the number of a formula $\mathrm{H}(n)$, then the assertion that *all* the formulae $\mathrm{H}(0)$, $\mathrm{H}(1)$, . . . are provable is expressed by

$$(\forall \mathrm{k})(\mathrm{E}m)\ \mathrm{Pr}(m, St_h(\nu/\mathrm{N}(k)))$$

and this formula may be provable in $\mathscr{A}$ even though $\mathrm{H}(n)$ is not. This constitutes a *formalisation of the notion of an arbitrarily assigned integer.*

The specific instance of an undecidable formula $(\forall m)\ \mathrm{G}(m)$ which is constructed in Gödel's proof is of no particular significance in arithmetic, but by formalising the proof of undecidability Gödel obtained the remarkable conclusion that the sentence of arithmetic which entails arithmetic's freedom from contradiction, viz.

$$(\forall m)(\forall r)(\forall s) \neg\, \{\mathrm{P}(r, m)\ \&\ \mathrm{P}(s, \mathrm{Neg}\ m)\} \qquad (\mathrm{C})$$

(where Neg m is the number of the negation of sentence number m) is itself undecidable, if arithmetic is consistent. Contrary to a widely held belief this result does not, however, establish the impossibility of proving the consistency of a codification of arithmetic by finitist methods formalisable within the codification. Even though the closed formula C is not provable in $\mathscr{A}$, each of its instances

$$\neg\, \mathrm{P}(\mathbf{r}, \mathbf{m})\ \&\ \mathrm{P}(\mathbf{s}, \mathrm{Neg}\ \mathbf{m})$$

is provable in $\mathscr{A}$ for arbitrary $\mathbf{r}$, $\mathbf{s}$, $\mathbf{m}$ (in virtue of Gentzen's consistency proof by transfinite induction), and the general formula expressing the provability of these instances may itself be provable in $\mathscr{A}$. But of course a proof inside $\mathscr{A}$ of $\mathscr{A}$'s consistency offers no security, for if $\mathscr{A}$ were inconsistent then every formula in $\mathscr{A}$ would be provable in $\mathscr{A}$.

In fact, Gödel's result does not really bear upon the problem of consistency itself but affords a means of establishing the independence from the axioms of $\mathscr{A}$ of axioms (like transfinite induction) whose addition to $\mathscr{A}$ suffice to prove the closed formula C above in the enlarged system.

If we add the unprovable formula $(\forall m)\ G(m)$ to $\mathscr{A}$ as a new axiom forming a system A^+ say, then exactly as before we can construct an undecidable formula in A^+, so that A^+ is also incomplete; even if we form a new system B, by adding in all the undecidable formulae of $\mathscr{A}$ as additional axioms, if the axioms of B form a recursive set, then B is still incomplete. Since $(\forall m)\ G(m)$ is not provable in $\mathscr{A}$ the system A^- formed by adding the *denial* of $(\forall m)\ G(m)$ is consistent if $\mathscr{A}$ is consistent, and so by Löwenheim's Theorem A^- must admit a denumerable model in which $G(0)$, $G(1)$, . . . are all true but $(\forall m)\ G(m)$ is false and therefore m must take values other than 0, 1, 2, . . .

It follows from this that A^- must admit what is called a non-standard model, that is, an interpretation in which a class of objects which is not ordinally similar to the natural numbers plays the part of the natural numbers. That this in fact is the case was shown independently by Skolem in 1934, who proved that a certain class of functions can play the number role.

The construction of the formula $G(m)$ above can also be carried out in a system without quantifiers, in some formalisation of recursive arithmetic, $\mathscr{R}$ say, and we find that $G(m)$ with free variable m is unprovable in $\mathscr{R}$, but each of $G(0)$, $G(1)$, . . . is provable. We cannot therefore explain away Gödel's incompleteness theorem as a defect of quantification theory. $\mathscr{R}$ like A is also incomplete. There is however an important difference between $\mathscr{R}$ and $\mathscr{A}$ since it can be shown that no non-standard model of $\mathscr{R}$ can itself be a recursive model.

Discussing Gödel's incompleteness theorem in 1934, before Skolem's result was known here, Wittgenstein was led to the same interpretation of the theorem, that induction and substitution of natural numbers for free variables fail to ensure that the natural numbers are the *only* values which the variables may take; it is perhaps surprising that the passages on Gödel's Theorem in the recently published 'Reflections on the Foundations of Mathematics' give no hint of this remarkable insight.

Since every axiom system for the natural numbers is incomplete and therefore necessarily admits a non-standard model it has been argued that we must look outside axiom systems for a logical foundation of arithmetic; this view is associated with a neo-realist outlook in foundation studies, that the elements of mathematics are objects in a real world with so-to-speak physical properties which the mathematician only partially captures in an axiom system. Certainly in some of his work the mathematician has an almost overwhelming conviction that he is uncovering connections which lie waiting to be revealed, but this may only reflect his amazement at the astonishing way the pieces of a puzzle sometimes fit together; we feel that the pieces must have been made to fit before we actually handled them, that we are only reconstructing a puzzle some other mind set for us. Neo-realism is not, however, a return to the Greek standpoint that formal geometry is an account of the space of our physical sensations. The case against classical philosophical realism in mathematics is overwhelmingly strong. In geometry the well-known consistency proofs of non-Euclidean geometry relative to the Euclidean makes it impossible for only one of the two geometries to be valid and yet both cannot mirror the real world. The neo-realist argues from this, not that the elements of geometry are *concepts*, but that no formal system can adequately express the whole of *geometry*, which is something revealed only to the intuition. This is an attractive thesis; every mathematician is conscious of possessing an 'inner sight', an inward short cut. But as a philosophical analysis of mathematics, neo-realism is no more tenable a position than classical realism. Intuition can be false and misleading and the inward 'short cut' just a *cul de sac*; intuition is certainly an important element in the creation of mathematics, but to see it as the organ which gives the mathematician access to mathematical reality is to be deceived by an analogy. When we say that no formal system can characterise the number concept, we do not mean that the number concept is something which we already have independently of the formal system; I may reject every definition of the meaning of a word, because it fails to characterise what I mean by the word, and maintain, rightly, that I know well what the meaning is, and yet my knowing what the meaning is may consist in nothing more than my rejection of the definitions. Just as I may write a story and be left with the feeling that this is not the story I meant to write, although of course I have not already in mind another story with which I compare it. When we contrast formal mathematics with intuitive mathematics we are not contrasting an image with reality, but a game played according to strict rules with a game with rules which change with the changing situation; a proof in intuitive mathematics may be a particular way of looking at a diagram, i.e. a particular way of using it as a symbol, or it may consist in stepping outside the particular system with which we are operating.

What we call an intuitive proof of some particular mathematical relation, is not a proof intelligible only to some special sense, quite the contrary. An intuitive proof of the relation is a proof which makes the minimum appeal to esoteric knowledge, which links the relation most immediately to a familiar background; but in its role as proof an intuitive proof has the same essential character as a formal proof, it exposes the connections between one relation and another.

An intuitive proof may for instance be a proof in which generality is expressed without the use of variables. For instance I may prove the general theorem $ab = ba$ without introducing variables, by looking at the array

$$\begin{array}{ccccccc} \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \\ \cdot & \cdot & \cdot & \cdot & \cdot & \cdot & \cdot \end{array}$$

first as five rows of seven dots and then as seven columns of five dots. What is *general* now is the *method* of proof; to show some one that the proof *is* general, it may be necessary to write it out again, with different numbers of dots, but this only means that we seek to draw attention to certain features of the proof, not that the proof appeals to a different *sense* than a proof which uses symbols for generality. The proof is just as formal as the proof with variables and quantifiers. What a single formal system is unable to do is to comprehend all possible partial systems in a single whole—only in this sense are formal systems necessarily incomplete; the only ' reality ' with which we can contrast a formal system is another system in a hierarchy of more or less uniformly formalised systems.

There is another incompleteness theorem for $\mathscr{A}$ which has no parallel in $\mathscr{R}$. It can be shown (for instance by means of the well-known result of classical analysis that a bounded monotonic increasing sequence is convergent) that there is a formula F(x) in $\mathscr{A}$ such that (Ex) F(x) is provable in $\mathscr{A}$, but none of F(0), F(1), F(2), . . . is provable. The formula F is this incompleteness theorem itself contains a quantifier, and no example has yet been found of a primitive recursive predicate R such that (Ex) R(x) is provable (in some formalisation of arithmetic) and yet none of R(0), R(1), R(2), . . . is provable.

Gödel's construction of an undecidable sentence in a formal system $\mathscr{A}$ utilises a detailed knowledge of the proof procedure of the system and the undecidability may be thought to reflect this structure. Kleene has however given a uniform process for finding an undecidable formula in every suitable consistent formal system. By means of Gödel arithmetisation one may determine a primitive recursive relation $T(z, x, y)$ such that for $z = 0, 1, 2, \ldots,$ $(Ey)\ T(z, x, y)$ enumerates (with repetitions) all relations of the form $(Ey)\ R(x, y)$ with general recursive R. Hence given any general recursive R we may determine r so that

$$(Ey)\ R(x, y) = (Ey)\ T(r, x, y).$$

Let S be a formal system in which every general recursive function may be expressed and evaluated and such that for any formula F(x)of S there is a general recursive relation $R_F(x, y)$ which holds only when y is the number of a proof of F(x) in S.

Let $t(x, y)$ be the representation in S of the primitive recursive relation $T(x, x, y)$, let $A(x)$ stand for $(\forall y) \neg\ t(x, y)$, and let r be the number given above such that

$$(Ey)\ R_A(x, y) = (Ey)\ T(r, x, y) \tag{3}$$

Then if S is consistent and ω-consistent, neither $\forall(r)$ nor $\neg\ A(r)$ is provable in S.

For if $A(r)$ is provable, let p be the number of its proof, so that $R_A(r, p)$ holds, whence by (3) there is an η such that $T(r, r, \eta)$ holds, and therefore $t(r, \eta)$ is provable; but if $A(r)$ is provable, $\neg\ t(r, \eta)$ is provable. This contradiction in S shows that $A(r)$ is not provable in S, and therefore $\neg\ (Ey)R_A(r, y) \equiv \forall(\eta) \neg\ T(r, r, \eta)$ holds; consequently $\neg\ t(r, y)$ is provable for each y, and therefore $(Ey)\ t(r, y)$ is unprovable, i.e. $\neg\ A(r)$ is unprovable.

Kleene's procedure can be applied only to systems with quantifiers and in this respect is less general than Gödel's original construction which is applicable also to a free variable system. It is perhaps also worth noting that even in Kleene's procedure the actual instance of an undecidable sentence is a function of the formal system being considered, since the constant r in $A(r)$ is determined by the proof predicate $R_A(x, y)$ and this of course varies from system to system. But the most interesting feature of the proof is the contrast of the semi-formal predicate $T(x, x, y)$ with its intended formal counterpart $t(x, y)$. In the theory of the predicate $T(x, x, y)$ we suppose we have before us a system of equations from which the value of a function $f(x)$ is derived by repeated substitution, a certain incompletely defined auxiliary function in the equations yielding a value only when we reach a value of y such that $R(x, y)$ holds; we then use Gödel numbering to replace the syntactical notion of deriving from a system of equations by a primitive recursive function (which we may for present purposes identify with) $T(r, x, y)$, r being the number of the system of equations and y the number of the derivation of an end equation $f(x) = \zeta$ from the system, the final step is to link $R(x, y)$ and $T(r, x, y)$ by the observation that there is a derivation of an end equation if and only if there is a y for which $R(x, y)$ holds. We are not concerned here with a formula in a formal system

$$(\mathrm{E}y)\, R(x, y) = (\mathrm{E}y)\, T(r, x, y)$$

but, as we say, with an assertion of *existence*. What does this mean? In this case that we have a procedure that enables us to *construct* the y for which $T(z, x, y)$ holds from a knowledge of the y for which $R(x, y)$ holds and conversely. But of course this *procedure* is a purely formal procedure. The situation is exactly akin to a familiar application of mathematics. If oranges cost 3d. each then I must pay 1/3d. for five; the reason for paying 1/3d. is the formal equation

$$5 \times 3 = 12 + 3,$$

but the actual purchase and payment lie outside the formal system. Depicting a computation procedure in this way (without necessarily using it) is one of the things we mean by an intuitive proof.

It is often said that Gödel's formula $(\forall m)\, G(m)$ is *true* but unprovable. The reason for saying that it is true is presumably that since each of $G(0)$, $G(1)$, $G(2)$, . . . is provable, and so true, therefore $G(m)$ is true for all m, which is just another way of saying that $(\forall m)\, G(m)$ is true. Of course if we do mean nothing more by saying that $(\forall m)\, G(m)$ is true than that $G(m)$ is true for all m then it is certainly true to say that $(\forall m)\, G(m)$ is true but unprovable. But the expression is a rather misleading one. The relationship between the formal system and the metalanguage which is established by recursion assures us that if $R(m)$ is a primitive recursive predicate such that $R(\mathbf{m})$ holds for some $\mathbf{m}$ then certainly $R(\mathbf{m})$ is provable in $\mathscr{A}$, or rather the formula in $\mathscr{A}$ which represents $R(\mathbf{m})$ is provable. But it is the essence of Gödel's theorem itself that although $G(m)$ is primitive recursive the formula $(\forall m)\, G(m)$ does *not* express the notion 'for all m, $G(m)$' in the formal system. As we have seen there is an interpretation of the system in which $G(0)$, $G(1)$, $G(2)$, . . . are *not* all the instances of $(\forall m)\, G(m)$ and therefore the truth of these instances is not to be identified with the truth of the formula $(\forall m)\, G(m)$.

Another common mistake is to suppose that $(\forall m)\ G(m)$ is true because it truly affirms of itself that it is non-demonstrable. We recall that $G(m)$ is an abbreviation for the formula which is obtained by substituting the number **a** of the formula

$$(\forall m) \neg \mathrm{Pr}(m, St_n(\mathbf{v}/n))$$

for the variable n in this formula.

The number of the resulting formula is of course

$$\mathrm{St}_{\mathbf{a}}\ (\mathbf{v}/\mathbf{a})$$

as we already have had occasion to remark; hence the fact that $(\forall m)$ $G(m)$ is unprovable, i.e. that formula number $\mathrm{St}_{\mathbf{a}}(\mathbf{v}/\mathbf{a})$ is unprovable, tells us that

$$\neg\ \mathrm{Pr}(m, \mathrm{St}_{\mathbf{a}}(\mathbf{v}/\mathbf{a}))$$

is provable for each value of m, but this of course tells us nothing about the formula

$$(\forall m) \neg \mathrm{Pr}(m, \mathrm{St}_{\mathbf{a}}(\mathbf{v}/\mathbf{a}))$$

that is, nothing about $(\forall m)\ G(m)$. The Gödel numbering establishes a code in which each instance of the numerical formula $G(\mathbf{m})$ says that $\mathbf{m}$ is not the number of the proof of $(\forall m)\ G(m)$, but $(\forall m)\ G(m)$ itself says nothing at all in the code. Thus the Gödel sentence is neither an example of self-reference nor of self-description. Even the sense in which we can say that the formula of $\mathscr{A}$ which we are denoting by

$$\mathrm{Pr}(\mathbf{x}, \mathbf{y}) \tag{P}$$

says that **x** is the number of the proof of formula number **y** is in need of clarification. As a formula of $\mathscr{A}$, P says nothing at all. Asthe representative in $\mathscr{A}$ of a certain arithmetical relation it says that y is the exponent of the greatest power of the greatest prime number which divides x; and only as a sentence of the code which the Gōdel numbering establishes, does this arithmetical relation say that **x** is the number of the proof of formula number **y**.

Even supposing, which is not in fact the case, that there is a formula of the formal system (let us call it ϕ) such that *as a sentence of the code* ϕ says something about the formula ϕ of the formal system, we still could not claim that ϕ is an example of successful self-reference or self-description, for as an element of the formal system ϕ is just a sign pattern, and as a sentence of the code ϕ refers not to itself i.e. not to its meaning, but to the sign by which it is expressed, in the way the sentence

‘ This is written in chalk ’

refers to its physical character, not to its sense.

What a sentence affirms depends upon the language in which the sentence is being used as a sentence, and identical sentences may express different propositions in different languages, as the obvious example of a code in which every English sentence stands for its contrary shows. But we must not, therefore, suppose, as many logicians do, by analogy with the specification of the range of a variable in mathematics, that every sentence *p* must be qualified by another sentence which names the language to which *p* belongs; for this assumption leads to an infinite hierarchy of languages, without achieving its aim. Whether we are speaking a common language or not cannot ultimately be settled by language alone but must show itself in our actions.

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PROOFS AND REFUTATIONS (III)*

I. LAKATOS

5 *Criticism of the Proof-analysis by Counterexamples which are Global but not Local. The Problem of Rigour.*

(*a*) *Monsterbarring in defence of the theorem*

GAMMA: I have just discovered that my *Counterexample 5*, the cylinder, refutes not only the naïve conjecture but also the theorem. Although it satisfies both lemmas, it is not Eulerian.

ALPHA: Dear Gamma, do not become a crank. The cylinder was a joke, not a counterexample. No serious mathematician will take the cylinder for a polyhedron.

GAMMA: Why didn't you protest against my *Counterexample 3*, the urchin? Was that less 'crankish' than my cylinder?[1] *Then* of course you were *criticising* the naïve conjecture and welcomed refutations. *Now* you are *defending* the theorem and abhor refutations! *Then*, when a counterexample emerged, your question was: *what is wrong with the conjecture? Now* your question is: *what is wrong with the counterexample?*

DELTA: Alpha, you have turned into a monsterbarrer! Aren't you embarrassed?[2]

(*b*) *Hidden lemmas*

ALPHA: I am. I may have been a bit rash. Let me think. There are *three possible types of counterexamples*. We have already discussed the *first*, which is local but not global—it certainly would not refute the theorem.[1] The *second*, which is both global and local, does not require any action: far from refuting the theorem, it confirms it.[2] Now there may be a *third* type, which is global but not local. This would refute the theorem. I did not think that this was possible. Now Gamma claims that the cylinder is one. If we do not want to reject it as a monster, we have to admit that it is a global counterexample: $V - E + F = 0$. But is it not of the second harmless type? I bet it does not satisfy at least one of the lemmas.

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* Part I and Part II appeared in the preceding numbers. The table of contents given with Part I has been slightly altered.

[1] The urchin and the cylinder were discussed previously in Part I, pp. 18 and 24.

[2] Monsterbarring in defence of the theorem is an important pattern in informal mathematics: 'What is wrong with the examples in which Euler's formula fails? Which geometrical conditions, rendering more precise the meaning of F, V, and E, would ensure the validity of Euler's formula?' (Pólya [1954], I, Exercise 29.) The cylinder is given in Exercise 24. The answer is: '. . . an edge . . . should terminate in corners . . .' (p. 225). Pólya formulates this generally: 'The situation, not infrequent in mathematical research is this: A theorem has been already formulated but we have to give a more precise meaning to the terms in which it is formulated in order to render it strictly correct' (p. 55).

GAMMA: Let us check. It certainly satisfies the first lemma: if I remove the bottom face, I can easily stretch the rest on to the blackboard.

ALPHA: But if you happen to remove the jacket, the thing falls into two pieces!

GAMMA: So what? The first lemma required that the polyhedron be 'simple', i.e. 'after having had a face removed, it can be stretched on to a plane'. The cylinder satisfies this requirement even if you start by removing the jacket. What you are claiming is that the cylinder should satisfy an *additional* lemma, namely that *the resulting plane network also be connected.* But who has ever stated *this* lemma?

ALPHA: Everybody has interpreted 'stretched' as 'stretched in *one piece*', 'stretched *without tear*'. . . . We decided not to incorporate the third lemma because of Epsilon's proof that it followed from the first two.[3] But just have a look at that proof: it hinges on the assumption that the result of the stretching is a *connected* network! Otherwise for the triangulated network $V - E + F$ would not be 1.

GAMMA: Why then didn't you insist on stating it *explicitly?*

ALPHA: Because we took it to be stated *implicitly*.

GAMMA: You, for one, certainly did not. For you proposed that 'simple' stand for 'pumpable into a ball'.[4] The cylinder *can* be pumped into a ball—so according to *your* interpretation it *does* comply with the first lemma.

ALPHA: Well. . . . But you have to agree that it does *not* satisfy the *second* lemma, namely, that '*any face dissected by a diagonal falls into two pieces*'. How will you triangulate the circle or the jacket? Are these faces simply-connected?

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[1] Local but not global counterexamples were discussed in Part I, pp. 11-14.

[2] This corresponds to the paradox of confirmation (Hempel [1945]).

[3] See Part II, p. 137

[4] See Part II, p. 130

GAMMA: Of course they are.

ALPHA: But on the cylinder one cannot draw diagonals at all! A diagonal is an edge that connects two non-adjacent vertices. But your cylinder has no vertices!

GAMMA: Don't get upset. If you want to show that the circle is not simply-connected, draw a diagonal which does *not* create a new face.

ALPHA: Don't be funny; you know very well that I cannot.

GAMMA: Then would you admit that 'there is a diagonal of the circle that does not create a new face' is a *false* statement?

ALPHA: Yes, I would. What are you up to now?

GAMMA: Then you are bound to admit that its negation is true, namely, that 'all diagonals of the circle create a new face', or, that 'the circle is simply-connected'.

ALPHA: You cannot give an *instance* of your lemma that 'all diagonals of the circle create a new face'—therefore it is not *true*, but *meaningless*. Your conception of truth is false.

KAPPA [*aside*]: First they quarrelled about what is a polyhedron, now about what is truth![1]

GAMMA: But you already admitted that the negation of the lemma was *false*! Or can a proposition *A* be *meaningless* while *Not-A* is *meaningful and false*? Your conception of meaning does not make sense!

Mind you, I see your difficulty; but we can overcome it by a slight reformulation. Let us call a face simply-connected if '*for all x, if x is a diagonal then x cuts the face into two*'. Neither the circle nor the jacket can have diagonals, so that in their case, whatever *x* is, the antecedent will always be false. Therefore the conditional will be instantiated by any object, and will be both meaningful and true. Or, both the circle and the jacket are simply-connected—the cylinder satisfies the second lemma.

ALPHA: No! If you cannot draw diagonals and thereby triangulate the faces, you will never arrive at a flat triangular network and you will never be able to conclude the proof. How can you then claim that the cylinder satisfies the second lemma? Don't you see that *there must be an existential clause* in the lemma? The correct interpretation of the simply-connectedness of a face must be: '*for all x, if x is a diagonal, then x cuts the face into two; and there is at least one x that is a diagonal*'. Our original formulation may not have spelt it out but it was there as an unconsciously made '*hidden assumption*'.[1] All the faces of the cylinder fail to meet it; therefore the cylinder is a counterexample which is *both* global and local, and it does *not* refute the theorem.

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[1] Gamma's *vacuously true statements* were a major innovation of the nineteenth century. Its problem-background has not yet been unfolded.

GAMMA: First you modified the stretching lemma by introducing 'connectedness', now the triangulating lemma by introducing your existential clause! And all this obscure talk about 'hidden assumptions' only hides the fact that my cylinder made you invent these modifications.

ALPHA: What obscure talk? We already agreed to omit, that is, 'hide', trivially true lemmas.[2] Why then should we state and incorporate trivially *false* lemmas—they are just as trivial and just as boring! Keep them in your mind (*en thyme*) but do not state them. A hidden lemma is not an error: it is shrewd shorthand pointing to our background knowledge.

KAPPA [*aside*]: Background knowledge is where we assume that we know everything but in fact know nothing.[3]

GAMMA: If you did make conscious assumptions, they were that (*a*) removing a face always leaves a connected network and (*b*) any

[1] 'Euclid employs an axiom of which he is wholly unconscious' (Russell [1903], p. 407). 'To make [*sic*] a hidden assumption' is a common phrase among mathematicians and scientists. See also Gamow's discussion of Cauchy's proof ([1953], p. 56) or Eves-Newsom on Euclid ([1958], p. 84).

[2] See Part II, pp. 137-8

[3] Good textbooks in informal mathematics usually specify their 'shorthand', i.e. those lemmas, either true or false, which they regard so trivial as not to be worth mentioning. The standard expression for this is 'we assume *familiarity* with lemmas of type *x*'. The amount of assumed familiarity decreases as criticism turns background knowledge into knowledge. Cauchy, e.g. did not even notice that his celebrated [1821] presupposed 'familiarity' with the *theory of real numbers*. He would have rejected as a monster any counterexample which made lemmas about the nature of irrational numbers explicit. Not so Weierstrass and his school: textbooks of informal mathematics now contain a new chapter on the theory of real numbers where these lemmas are collected. But in their introductions 'familiarity with the *theory of rational numbers*' is usually assumed. (See e.g. Hardy's *Pure Mathematics* from the second edition (1914) onwards—the first edition still relegated the theory of real numbers to background knowledge; or Rudin [1953]). More rigorous textbooks narrow down background knowledge even further: Landau, in the introduction to his famous [1930], assumes familiarity only with '*logical reasoning and German language*'. It is ironical that at the very same time Tarski showed that the absolutely trivial lemmas thus omitted may not only be false but inconsistent—German being a semantically closed language. One wonders when 'the author confesses ignorance about the field *x*' will replace the authoritarian euphemism 'the author assumes familiarity with the field *x*': surely only when it is recognised that knowledge has no foundations.

non-triangular face can be dissected into triangles by diagonals. While they were in your *subconscious*, they were listed as *trivially true*—the cylinder however made them somersault into your *conscious* list as *trivially false*. Before being confronted by the cylinder you could not even conceive that the two lemmas could be false. If you now say that you did, then you are rewriting history to purge it from error.[1]

THETA: Not long ago, Alpha, you ridiculed the ' hidden ' clauses which cropped up in Delta's definitions after each refutation. Now it is you who make up ' hidden ' clauses in the lemmas after each refutation, it is you who shift your ground and try to hide it to save face. Aren't you embarrassed?

KAPPA: Nothing amuses me more than the dogmatist at bay. After donning the militant sceptic's robe to demolish a lesser brand of dogmatism, Alpha becomes frantic when *he* in turn is cornered by the same sort of sceptical arguments. He now plays fast and loose: trying to fight off Gamma's counterexample first with the defence-mechanism he himself had exposed and forbidden (monsterbarring), then by smuggling a reserve of ' hidden lemmas ' into the proof and corresponding ' hidden conditions ' into the theorem. What is the difference?

TEACHER: The trouble with Alpha was certainly the dogmatist turn in his interpretation of lemma-incorporation. He thought that a careful inspection of the proof would yield a perfect proof-analysis containing *all* the false lemmas (just as Beta thought he could enumerate *all* the exceptions). He thought that by incorporating them he could attain not only an improved theorem, but a *perfected* theorem,[1] *without bothering about counterexamples.* The cylinder showed him to be wrong but, instead of admitting it, he now wants to call a proof-analysis complete if it contains all the *relevant* false lemmas.

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[1] When it is first discovered, the hidden lemma is considered an error. When J. C. Becker first pionted out a ' hidden ' (*stillschweigend*) assumption in Cauchy's proof (he quoted the proof second-hand from Baltzer's [1806-27]), he called it an ' *error* ' ([1869], pp. 67-68). He drew attention to the fact that Cauchy thought that *all* polyhedra were simple: his lemma was not only hidden but also false. Historians however cannot imagine that great mathematicians should make such errors. A veritable programme of how to falsify history can be found in Poincaré's [1908]: ' A demonstration which is not rigorous is nothingness. I think no one will contest this truth. But if it were taken too literally, we should be led to conclude that before 1820, for example, there was no mathematics; this would be manifestly excessive; the geometers of that time understood voluntarily what we explain by prolix discourse. This does not mean that they did not see it at all; but they passed over it too rapidly, and to see it well would have necessitated taking the pains to say it ' (p. 374). Becker's report about Cauchy's ' error ' had to be rewritten 1984-wise: ' doubleplusungood refs unerrors rewrite fullwise.' The rewriting was done by E. Steinitz who insisted that ' the fact that the theorem was not generally valid could not possibly remain unnoticed ' ([1914-31], p. 20). Poincaré himself applied his programme to the Euler-theorem: ' It is known that Euler proved that $V - E + F = 2$ for *convex* polyhedra ' ([1893])—Euler of course stated his theorem for *all* polyhedra.

(c) *The method of proof and refutations*

GAMMA: I propose to accept the cylinder as a genuine counterexample to the theorem. I invent a new lemma (or lemmas) that will be refuted by it and add the lemma(s) to the original list. This of course is exactly what Alpha did. But instead of 'hiding' them so that they *become* hidden, I announce them publicly.

Now the cylinder which was a puzzling, dangerous global but not local counterexample (the third type) in respect of the old proof-analysis and of the corresponding old theorem, will be a harmless, global *and* local counterexample (the second type) in respect of the new proof-analysis and the corresponding new theorem.

Alpha thought that his classification of counterexamples was absolute—but in fact it was relative to his proof-analysis. As proof-analysis grows, counterexamples of the third type turn into counterexamples of the second type.

LAMBDA: That is right. A proof-analysis is 'rigorous' or 'valid' and the corresponding mathematical theorem true if, and only if, there is no 'third-type' counterexample to it. I call this criterion the *Principle of Retransmission of Falsity* because it demands that global counterexamples be also local: falsehood should be retransmitted from the naïve conjecture to the lemmas, from the consequent of the theorem to its antecedent. If a global but not local counterexample violates this principle, we restore it by adding a suitable lemma to the proof-analysis. The Principle of Retransmission of Falsity is therefore a *regulative principle* for proof-analysis in *statu nascendi*, and a global but not local counterexample is a fermenting agent in the growth of proof-analysis.

GAMMA: Remember, even before finding a single refutation we managed to pick out three suspicious lemmas and go ahead with the proof-analysis!

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[1] See Part II, p. 138

LAMBDA: That is true. Proof-analysis may start not only under the pressure of global counterexamples but also when people have already learned to be on guard against ' convincing ' proofs.[1]

In the *first case* all global counterexamples appear as counterexamples of the third-type, and all the lemmas start their career as ' hidden lemmas '. They lead us to a gradual build-up of the proof-analysis and so turn one by one into counterexamples of the second-type.

In the *second case*—when we are already in a suspicious mood and look out for refutations—we may arrive at an advanced proof-analysis without any counterexamples. Then there are two possibilities. The *first possibility* is that *we succeed* in refuting—by local counterexamples—the lemmas listed in our proof-analysis. We may very well find that these are also global counterexamples.

ALPHA: This is how I discovered the picture-frame: looking for a polyhedron that, after having a face removed, could not be stretched flat onto a plane.

SIGMA: Then not only do refutations act as fermenting agents for proof-analysis, but proof-analysis may act as a fermenting agent for refutations! What an unholy alliance between seeming enemies!

LAMBDA: That is right. If a conjecture seems very plausible or even self-evident, one should prove it: one may find that it hinges on very sophisticated and dubious lemmas. Refuting the lemmas may lead to some unexpected refutation of the original conjecture.

SIGMA: To proof-generated refutations!

GAMMA: Then ' the virtue of a logical proof is not that it compels belief, but that it suggests doubts '.[2]

LAMBDA: But let me come back to the *second possibility*: when we do *not* find any local counterexamples to the suspected lemmas.

SIGMA: That is, when refutations do not assist proof-analysis! What would happen then?

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[1] Our class was a rather advanced one—Alpha, Beta, and Gamma suspected three lemmas when no global counterexamples turned up. In actual history proof-analysis came many decades later: for a long period the counterexamples were either hushed up or exorcised as monsters, or listed as exceptions. The heuristic move from the global counterexample to proof-analysis—the application of the Principle of Retransmission of Falsity—was virtually unknown in the informal mathematics of the early nineteenth century.

[2] H. G. Forder [1927], p. viii. Or: ' It is one of the chief merits of proofs that they instil a certain scepticism as to the result proved.' (Russell [1903], p. 360. He also gives an excellent example.)

LAMBDA: We would be branded cranks. The proof would acquire absolute respectability and the lemmas would shake off suspicion. Our proof-analysis would soon be forgotten.[1] Without refutations one cannot sustain suspicion: the searchlight of suspicion soon switches off if a counterexample does not reinforce it, directing the limelight of refutation onto a neglected aspect of the proof that had scarcely been noticed in the twilight of 'trivial truth'.

All this shows that one cannot put proof and refutations into separate compartments. This is why I would propose to rechristen our '*method of lemma-incorporation*' the '*method of proof and refutations*'. Let me state its main aspects in three heuristic rules:

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[1] It is well known that *criticism* may cast doubt on, and eventually refute, '*a priori* truths' and so turn *proofs* into mere *explanations*. That *lack of criticism or of refutation* may turn implausible conjectures into '*a priori* truths' and so tentative explanations into proofs is not so well-known but just as important. Two major examples of this pattern are the emergence and fall of Euclid and Newton. The story of their fall is well-known, but the story of their emergence is usually misrepresented.

Euclid's geometry seems to have been proposed as a cosmological theory (cf. Popper [1952], pp. 147-148). Both its 'postulates' and 'axioms' (or 'common notions') were proposed as bold, provocative propositions, challenging Parmenides and Zeno, whose doctrines entailed not only the falsity, but even the logical falsity, the inconceivability, of these 'postulates'. Only later were the 'postulates' taken to be indubitably true and the bold anti-Parmenidean 'axioms' (such as 'the whole is greater than the part') taken to be so trivial that they were omitted in later proof-analysis and turned into 'hidden lemmas'. This process started with Aristotle: he branded Zeno a quarrelsome crank, and his arguments 'sophistry'. This story was recently unfolded in exciting detail by Árpád Szabó ([1960], pp. 65-84). Szabó showed that in Euclid's time the word 'axiom'—like 'postulate'—meant a proposition in the critical dialogue (dialectic) put forward to be tested for consequences *without* being admitted as true by the discussion-partner. It is the irony of history that its meaning was turned upside down. The peak of Euclid's authority was reached in the Age of Enlightenment. Clairaut urges his colleagues not to 'obscure proofs and disgust readers' by stating evident truths: Euclid did so only in order to convince 'obstinate sophists' ([1741], pp. x and xi).

Again, *Newton's mechanics and theory of gravitation* was put forward as a daring guess, which was ridiculed and called 'occult' by Leibnitz and suspected even by Newton himself. But a few decades later—in the absence of refutations—his axioms came to be taken as indubitably true. Suspicions were forgotten, critics branded 'eccentric' if not 'obscurantist'; some of his most doubtful assumptions came to be regarded as so trivial that textbooks never even stated them. The debate—from Kant to Poincaré—was no longer about the truth of Newtonian theory but about the nature of its certainty. (This *volteface* in the appraisal of Newtonian theory was first pointed out by Karl Popper—see his [1963], *passim*.)

The analogy between political ideologies and scientific theories is then more far-reaching than is commonly realised: political ideologies which first may be debated (and perhaps accepted only under pressure) may turn into unquestioned background knowledge even in a single generation: the critics are forgotten (and perhaps executed) until a revolution vindicates their objections.

Rule 1. *If you have a conjecture, set out to prove it and to refute it. Inspect the proof carefully to prepare a list of non-trivial lemmas (proof-analysis); find counterexamples both to the conjecture (global counterexamples) and to the suspect lemmas (local counterexamples).*

Rule 2. *If you have a global counterexample discard your conjecture, add to your proof-analysis a suitable lemma that will be refuted by it, and replace the discarded conjecture by an improved one that incorporates that lemma as a condition.*[1] *Do not allow a refutation to be dismissed as a monster.*[2] *Make all 'hidden lemmas' explicit.*[3]

Rule 3. *If you have a local counterexample, check to see whether it is not also a global counterexample. If it is, you can easily apply Rule* 2.

(*d*) *Proof versus proof-analysis. The relativisation of the concepts of theorem and rigour in proof-analysis*

ALPHA: What did you mean by 'suitable' in your *Rule* 2?

GAMMA: It is completely redundant. *Any* lemma which is refuted by the counterexample in question can be added—for *any* such lemma will restore the validity of the proof-analysis.

LAMBDA: What! So a lemma like 'All polyhedra have at least 17 edges' would take care of the cylinder! And any other random *ad hoc* conjecture would do just as well, so long as it happened to be refuted by the counterexample.

GAMMA: Why not?

LAMBDA: We already criticised monster-barrers and exception-barrers for forgetting about proofs.[4] Now you are doing the same, inventing a real monster: *proof-analysis without proof*! The only difference between you and the monsterbarrer is that you would have Delta make his arbitrary definitions explicit and incorporate them into the theorem as lemmas. And there is *no* difference between exception-barring and your proof-analysing. The only safeguard against such *ad hoc* methods is to use *suitable* lemmas, i.e. lemmas in accordance with the spirit of the thought-experiment! Or would you drop the beauty of the proofs from mathematics and replace it by a silly formal game?

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[1] This rule seems to have been stated for the first time by Ph. L. Seidel ([1847], p. 383).

[2] 'I have the right to put forward any example that satisfies the conditions of your argument and I strongly suspect that what you call bizarre, preposterous examples are in fact embarrassing examples, prejudicial to your theorem' (G. Darboux [1874]).

[3] 'I am terrified by the hoard of implicit lemmas. It will take a lot of work to get rid of them' (G. Darboux [1883]).

[4] See Part II, 125 and 133

GAMMA: Better than your 'spirit of the thoughtexperiment'! I am defending the objectivity of mathematics against your psychologism.

ALPHA: Thank you, Lambda, you restated my case: one does not *invent* a new lemma out of the blue to cope with a global but not local counterexample: rather, one inspects the proof with increased care and *discovers* the lemma there. So I did not, dear Theta, 'make up' hidden lemmas, and I did not, dear Kappa, 'smuggle' them into the proof. The proof contains all of them—but a mature mathematician understands the entire proof from a brief outline. We should not confuse *infallible proof* with *inexact proof-analysis*. There is still the irrefutable master-theorem: '*All polyhedra on which one can perform the thought-experiment*, or briefly, *all Cauchy-polyhedra, are Eulerian*.' My approximate proof-analysis drew the borderline of the class of Cauchy-polyhedra with a pencil that—I admit—was not particularly sharp. Now eccentric counterexamples teach us to sharpen our pencil. But first: *no pencil is absolutely sharp* (and if we overdo sharpening it will break); secondly, *pencil-sharpening is not creative mathematics*.

GAMMA: I am lost. What *is* your position? First you were a champion of refutations.

ALPHA: Oh, my growing pains! Mature intuition brushes controversy aside.

GAMMA: Your first mature intuition led you to your 'perfect proof-analysis'. You thought that your 'pencil' was absolutely sharp.

ALPHA: I forgot about the difficulties of linguistic communication—especially with pedants and sceptics. But the heart of mathematics is the thought-experiment—the proof. Its linguistic articulation—the proof-analysis—is necessary for communication but irrelevant. I am interested in polyhedra, you in language. Don't you see the poverty of your counterexamples? They are linguistic, not polyhedral.

GAMMA: Then refuting a theorem only betrays our failure to grasp the hidden lemmas in it? So a 'theorem' is meaningless unless we understand its proof?

ALPHA: Since the vagueness of language makes the *rigour of proof-analysis* unattainable, and turns theorem-formation into an unending process, why bother about the theorem? Working mathematicians certainly do not. If yet another petty ' counterexample ' is produced they do not admit that their theorem is refuted, but at most that its ' domain of validity ' should be suitable narrowed down.

LAMBDA: So you are not interested either in counterexamples, or in proof-analysis, or in lemma-incorporation?

ALPHA: That is right. I reject all your rules. I propose one single rule instead: *Construct rigorous (crystal-clear) proofs.*

LAMBDA: You argue that the *rigour of proof-analysis* is unattainable. Is *the rigour of proof* attainable? Cannot ' crystal-clear ' thought-experiments lead to paradoxical or even contradictory results?

ALPHA: Language is vague, but thought can achieve absolute rigour.

LAMBDA: But surely ' at each stage of the evolution our fathers also thought they had reached it? If they deceived themselves, do we not likewise cheat ourselves? '[1]

ALPHA: ' Today absolute rigour is attained.'[2]

[*Giggling in the classroom.*[3]]

GAMMA: This theory of ' crystal-clear ' proof is sheer psychologism![4]

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[1] Poincaré [1905], p. 214

[2] *Ibid.* p. 216. Changes in the criterion of ' rigour of the proof ' engender major revolutions in mathematics. Pythagoreans held that rigorous proofs can only be arithmetical. They however discovered a rigorous proof that $\sqrt{2}$ was ' irrational '. When this scandal eventually leaked out, the Criterion was changed: arithmetical ' intuition ' was discredited and geometrical intuition took its place. This meant a major and complicated reorganisation of mathematical knowledge (e.g. the theory of proportions). In the eighteenth century ' misleading ' figures brought geometrical proofs into disrepute, and the nineteenth century saw arithmetical intuition re-enthroned with the help of the cumbersome theory of real numbers. Today the main dispute is about what is rigorous and what not in set-theoretical and meta-mathematical proofs, as shown by the well-known discussions about the admissibility of Zermelo's and Gentzen's thoughtexperiments.

[3] As was already pointed out, the class is very advanced.

[4] The term ' psychologism ' was coined by Husserl ([1900]). For an earlier ' criticism ' of psychologism see Frege [1893], pp. xv–xvi. Modern intuitionists (unlike Alpha) openly embrace psychologism: ' A mathematical theorem expresses a purely empirical fact, namely the success of a certain construction . . . mathematics is a study of certain functions of the human mind ' (Heyting [1956], pp. 8 and 10). How they reconcile psychologism with certainty is their well-kept secret.

ALPHA: Better than the logico-linguistic pedantry of your proof-analysis![1]

LAMBDA: Swearwords apart, I too am sceptical about your conception of mathematics as 'an essentially languageless activity of the mind'.[2] How can an activity be true or false? Only *articulated* thought can try for truth. Proof cannot be enough: we also have to state what the proof proved. The proof is only a stage of the mathematician's work which has to be followed by proof-analysis and refutations and concluded by the rigorous theorem. We have to *combine* the 'rigour of proof' with the 'rigour of proof-analysis'.

ALPHA: Are you still hoping that at the end you will arrive at a perfectly rigorous proof-analysis? If so, tell me why you did not start by formulating your new theorem 'stimulated' by the cylinder? You only indicated it. Its length and clumsiness would have made us laugh in despair. And this only after the *first* of your new counterexamples! You replaced our original theorem by a succession of ever more precise theorems—but only *in theory*. What about the *practice* of this relativisation? Ever more eccentric counterexamples will be countered by ever more trivial lemmas—yielding a 'vicious infinity'[3] of ever longer and clumsier theorems.[4] If criticism was felt to be invigorating while it seemed to lead to truth, now it is certainly frustrating when it destroys any truth whatsoever and drives us endlessly without purpose. I stop this vicious infinity in *thought*—you will never stop it in *language*.

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[1] That even if we had perfect knowledge we could not perfectly articulate it, was a commonplace for ancient sceptics (see Sextus Empiricus [*c.* 195], I. 83-87), but was forgotten in the Enlightenment. It was rediscovered by the intuitionists: they accepted Kant's philosophy of mathematics but pointed out that 'between the perfection of mathematics proper and the perfection of mathematical language no clear connection can be seen' (Brouwer [1952], p. 140). 'Expression by spoken or written word—though necessary for communication—is never adequate . . . The task of science is not to study languages, but to create ideas' (Heyting [1939], pp. 74-75).

[2] Brouwer [1952], p. 141

[3] English has the term '*infinite regress*', but this is only a *special* case of 'vicious infinity' (*schlechte Unendlichkeit*) and would not apply here. Alpha obviously coined this phrase with '*vicious circle*' in mind.

[4] Usually mathematicians avoid long theorems by the alternative device of long definitions, so that in the theorems only the defined terms (e.g. 'ordinary polyhedron') appear—this is more economical since one definition abbreviates many theorems. Even so, the definitions take up enormous space in 'rigorous' expositions, though the monsters which lead to them are seldom mentioned. The definition of an '*Euler polyhedron*' (with the definitions of some of the defining terms) takes about 25 lines in Forder [1927] (pp. 67 and 29); the definition of '*ordinary polyhedron*' in the 1962 edition of the *Encyclopaedia Britannica* fills 45 lines.

GAMMA: But I never said that there have to be *infinitely many* counterexamples. At a certain point we may reach truth and then the flow of refutations will stop. But of course we shall not know when. Only refutations are conclusive—proofs are a matter of psychology.[1]

LAMBDA: I still trust that the light of absolute certainty will flash up when refutations peter out!

KAPPA: But will they? What if God created polyhedra so that all true universal statements about them—formulated in human language—are infinitely long? Is it not blasphemous anthropomorphism to assume that (divine) true theorems are of finite length?

Be frank: for some reason or other you are all bored with refutations and piecemeal theorem-formation. Why not call it a day and stop the game? You already gave up '*Quod erat demonstrandum*'. Why not give up '*Quod erat demonstratum*' too? Truth is only for God.

THETA [*aside*]: A religious sceptic is the worst enemy of science!

SIGMA: Let's not overdramatise! After all, only a narrow penumbra of vagueness is at stake. It is simply that, as I said before, *not all propositions are true or false*. There is a third class which I would now call '*more or less rigorous*'.

THETA [*aside*]: Three-valued logic—the end of critical rationality!

SIGMA: . . . and we state their domain of validity with a rigour that is more or less adequate.

ALPHA: Adequate for what?

SIGMA: Adequate for the solution of the problem which we want to solve.

THETA [*aside*]: Pragmatism! Has everybody lost interest in *truth*?

KAPPA: Or adequate for the *Zeitgeist*! 'Sufficient unto the day is the rigour thereof.'[2]

THETA: Historicism! [*Faints*].

ALPHA: Lambda's rules for '*rigorous proof-analysis*' deprive mathematics of its beauty, present us with the hairsplitting pedantry of long, clumsy theorems filling dull thick books, and will eventually land us in vicious infinity. Kappa's escape-route is convention, Sigma's mathematical pragmatism. What a choice for a rationalist!

GAMMA: So a rationalist ought to relish Alpha's '*rigorous proofs*', inarticulate intuition, 'hidden lemmas', derision of the Principle of

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[1] 'Logic makes us reject certain arguments, but it cannot make us believe any argument' (Lebesgue [1928], p. 328).

[2] E. H. Moore [1902], p. 411

Retransmission of Falsity, and elimination of refutations? Should mathematics have no relation to criticism and logic? [1]

BETA: Whatever the case, I am fed up with all this inconclusive verbal quibble. I want to do mathematics and I am not interested in the philosophical difficulties of justifying its foundations. Even if reason fails to provide such justification my natural instinct reassures me.[1]

I understand Omega has an interesting collection of alternative proofs—I would rather listen to him.

OMEGA: But I shall put them into a 'philosophical' framework!

BETA: I don't mind packing if there is something else in the packet.

Note. In this section I have tried to show how the emergence of mathematical criticism has been the driving force in the search for the 'foundations' of mathematics.

The distinction that we made between *proof* and *proof-analysis* and the corresponding distinction between the *rigour of proof* and the *rigour of proof-analysis* seems to be crucial. About 1800 the *rigour of proof* (crystal-clear thought experiment or construction) was contrasted with muddled argument and inductive generalisation. This was what Euler meant by '*rigida demonstratio*', and Kant's idea of infallible mathematics too was based on this concept (see his paradigm case of a mathematical proof in his [1781], pp. 716-717). It was also thought that one proves what one has set out to prove. It did not occur to anybody that the verbal articulation of a thought-experiment involves any real difficulty. Aristotelian formal logic and mathematics were two completely separate disciplines—mathematicians considered the former as utterly useless. The proof or thoughtexperiment carried full conviction without any deductive pattern or 'logical' structure.

In the early nineteenth century the flood of counterexamples brought confusion. Since proofs were crystal-clear, refutations had to be miraculous freaks, to be completely segregated from the indubitable proofs. *Cauchy's revolution of rigour* rested on the heuristic innovation that the mathematician should not stop at the proof: he should go on and find out what he has proved by enumerating the exceptions, or rather by stating a safe domain where the proof is valid. *But Cauchy—or Abel—did not see any connection*

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[1] 'Nature confutes the sceptics, reason confutes the dogmatists' (Pascal [1654], 432). Few mathematicians would confess—like Beta—that reason is too weak to justify itself. Most of them adopt some brand of dogmatism, historicism or confused pragmatism and remain curiously blind to its untenability; for example: 'Mathematical truths are in fact *the prototype of the completely incontestable*. . . . But the rigor of maths is not absolute; it is in a process of continual development; the *principles of maths have not congealed once and for all* but have a life of their own and may even be the subject of scientific quarrels' (A. D. Alexandrov [1947], p. 7). (This quotation may remind us that dialectic tries to account for change without using criticism: truths are in 'in continual development' but always 'completely incontestable'.)

between the two problems. It never occurred to them that if they discover an exception, they should have another look at the proof. (Others practised monster-barring, monster-adjustment or even ' turning a blind eye '—but all agreed that the proof was taboo and had nothing to do with the ' exceptions '.)

The nineteenth century union of logic and mathematics had two main sources: Non-Euclidean geometry and the *Weierstrassian revolution of rigour.* They brought about the integration of proof (thoughtexperiment) and refutations and started to develop *proof-analysis*, gradually introducing deductive patterns in the proof-thoughtexperiment. What we called the ' method of proof and refutations ' was their heuristic innovation: *it united logic and mathematics for the first time.* Weierstrassian rigour triumphed over its reactionary monster-barring and lemma-hiding opponents who used slogans like ' the dullness of rigour ', ' artificiality versus beauty ', etc. *The rigour of proof-analysis superseded the rigour of proof*; but most mathematicians put up with its pedantry only so long as it promised them complete certainty.

Cantor's set-theory—with yet another crop of unexpected refutations of ' rigorous ' theorems—turned many of the Weierstrassian Old Guard into dogmatists, ever ready to combat the ' anarchists ' by barring the new monsters or referring to ' hidden lemmas ' in their theorems which represented ' the last word in rigour ' while still chastising the older type ' reactionaries ' for like sins.

Then some mathematicians realised that the drive for rigour of proof-analysis in the method of proofs and refutations leads to vicious infinity. An ' intuitionist ' counter-revolution began: the frustrating logico-linguistic pedantry of *proof-analysis* was condemned, and new extremist standards of rigour were invented for *proofs*; mathematics and logic were divorced once more.

Logicists tried to save the marriage and foundered on the paradoxes. Hilbertian rigour turned mathematics into a cobweb of *proof-analyses* and claimed to stop their infinite regresses by crystal-clear consistency *proofs* of his intuitionistic metatheory. The ' foundational layer ', the region of uncriticisable familiarity, was shifted into the thoughtexperiments of metamathematics. (Cf. Lakatos [1962], pp. 179-184).

By each ' revolution of rigour ' proof-analysis penetrated deeper into the proofs down to the *foundational layer* of ' familiar background knowledge ' (also cf. footnote 3, p. 224), where crystal-clear intuition, the rigour of the proof, reigned supreme and criticism was banned. Thus, *different levels of rigour differ only about where they draw the line between the rigour of proof-analysis and the rigour of proof*, i.e. *about where criticism should stop and justification should start.* ' Certainty is never achieved '; ' foundations ' are never found—but the 'cunning of reason' turns each increase in *rigour* into an increase in *content*, in the scope of mathematics. But this story is beyond our present investigation.

6 Return to the Criticism of the Proof by Counterexamples which are Local but not Global. The Problem of Content.

(a) Increasing Content by Deeper Proofs

OMEGA: I like Lambda's method of proof and refutations and I share his faith that somehow we shall finally arrive at a rigorous proof-analysis and thereby at a certainly true theorem. But even so, our very method creates a new problem: *proof-analysis, when increasing certainty, decreases content.* Each new lemma in the proof-analysis, each corresponding new condition in the theorem, reduces its domain. Increasing rigour is applied to a decreasing number of polyhedra. Does lemma-incorporation not repeat the mistake Beta made in playing for safety? Could we too 'have withdrawn too radically, leaving lots of Eulerian polyhedra outside the walls?' [1] In both cases we may throw the baby out with the bathwater. *We should have a counterweight against the content-decreasing pressure of rigour.*

We have already made a few steps in this direction. Let me remind you of two cases and re-examine them.

One was when we first came across local but not global counterexamples.[2] Gamma refuted the third lemma in our first proof-analysis (that 'in removing triangles from the flat triangulated network we have only two possibilities: either we remove an edge or we remove two edges and a vertex'). He removed a triangle from the middle of the network without removing a single edge or vertex.

We then had two possibilities.[3] The *first* was to incorporate the false lemma into the theorem. This would have been a perfectly proper procedure as far as certainty is concerned, but would have reduced the domain of the theorem so drastically that it would have applied only for the tetrahedron. Together with the counterexamples we would have thrown out all the examples but one.

This was the rationale behind our adoption of the alternative: instead of *narrowing* the domain of the theorem by lemma-incorporation, we *widened* it by replacing the falsified lemma by an unfalsified one. But this vital pattern for theorem-formation was soon forgotten and Lambda did not bother to formulate it as a heuristic rule. It should be:

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[1] Part II, p. 125

[2] For the discussion of this first case see Part I, pp. 11-14.

[3] Omega seems to ignore a third possibility: Gamma may very well claim that since local but not global counterexamples do not show up any violation of the principle of retransmission of falsity, there is no action to be taken.

Rule 4. *If you have a counterexample which is local but not global try to improve your proof-analysis by replacing the refuted lemma by an unfalsified one.*

Counterexamples of the first type (local but not global) may provide an opportunity of *increasing* the content of our theorem which is constantly being *reduced* under the pressure of counterexamples of the third type (global but not local).

GAMMA: *Rule* 4 shows up again the weakness of Alpha's now discarded 'perfect proof-analysing intuition'.[1] He would have listed the suspicious lemmas, incorporated them immediately and—without caring for counterexamples—formed near-empty theorems.

TEACHER: Omega, let us hear the second example you promised.

OMEGA: In Beta's proof-analysis the second lemma was that '*all faces are triangular*'.[2] This can be falsified by a number of local but not global counterexamples, e.g. by the cube or the dodecahedron. Therefore you, Sir, replaced it by a lemma which is not falsified by them, namely that '*any face dissected by a diagonal edge falls into two pieces*'. But instead of invoking *Rule* 4 you rebuked Beta for 'careless proof-analysis'. You will admit that *Rule* 4 is better advice than just 'be more careful'.

BETA: You are right, Gamma, and you also make me understand better 'the method of the best sort of exceptionbarrers'.[3] They start with a cautious, 'safe' proof-analysis and systematically applying *Rule 4* they gradually build up the theorem without uttering a falsehood. After all, it is a matter of temperament whether one approaches truth through ever false overstatements or through ever true understatements.

OMEGA: That may be right. But one can interpret *Rule 4* in two ways. Hitherto we considered only the first, weaker interpretation: 'one *easily* elaborates, improves the proof by replacing the false lemma by a *slightly modified* one which the counterexample will not refute';[4] all that one needs for this is a 'more careful' inspection of the proof and a 'trifling observation'.[5] On this interpretation *Rule 4* is just local patching *within the framework of the original proof.*

I allow also for the alternative, radical interpretation: to replace the lemma—or possibly all the lemmas—not only by trying to squeeze out the last drop of content from the given proof, but possibly by inventing a completely different, more embracing, *deeper* proof.

TEACHER: For example?

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[1] Cf. pp. 225–6. [2] For the discussion of this second case cf. Part II, pp.132–4

[3] See *Part II*, pp. 134–135 [4] Part I, p. 12 [5] *Ibid.*

OMEGA: I discussed the Descartes-Euler conjecture earlier with a friend who immediately offered a proof, as follows: let us imagine the polyhedron to be hollow, with a surface made of any rigid material, say cardboard. The edges must be clearly painted on its inside. Let the inside be well illuminated, and let one of the faces be the lens of an ordinary camera—that face from which I can take a snapshot showing all edges and vertices.

SIGMA [*aside*]: A camera in a mathematical proof?

OMEGA: So I get a picture of a plane network, which can be dealt with just like the plane network in your proof. Also in the same way, I can show that, if the faces are simply-connected, $V - E + F = 1$, and adding the lens-face which is invisible on the photo, I get Euler's formula. The main lemma is that there is a face of the polyhedron which, if transformed into the lens of a camera, photographs the inside of the polyhedron so that all the edges and all the vertices are on the film. Now I introduce the following abbreviation: instead of ' a polyhedron which has at least one face from which we can photograph *all* the inside ', I shall say ' a quasi-convex polyhedron '.

BETA: So your theorem will be: All quasi-convex polyhedra with simply-connected faces are Eulerian.

OMEGA: For brevity and to give credit to the inventor of this particular proof-idea I would rather say: '*All Gergonne-polyhedra are Eulerian*'.[1]

GAMMA: But there are many simple polyhedra which, although perfectly Eulerian, are so badly indented that they have no face from which the whole of the inside can be photographed! Gergonne's proof is not deeper than Cauchy's—it is Cauchy's that is deeper than Gergonne's!

OMEGA: Of course! I suppose Teacher knew about Gergonne's proof, found out that it was unsatisfactory by some local but not global counterexample and replaced the optical—photographing—lemma by the wider topological—stretching—lemma. Thereby, he arrived at the *deeper* Cauchy proof, not by a 'careful proof-analysis' followed by a slight alteration, but by a radical, imaginative innovation.

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[1] Gergonne's proof is to be found in Lhuilier [1812–13], pp. 177–9. In the original it could not of course contain photographic devices. It says: ' Take a polyhedron, one of its faces being transparent; and imagine that the eye approaches this face from the outside, so closely, that it can perceive the inside of all the other faces . . . ' Gergonne points out modestly that Cauchy's proof is deeper, it ' has the valuable advantage that it does not assume convexity at all '. (It does not occur to him however to ask what it *does* assume.) Jacob Steiner later rediscovered essentially the same proof ([1826]). His attention was then called to Gergonne's priority, so he read Lhuilier's paper with the list of exceptions but this did not prevent him from concluding his proof with the ' theorem ': '*All polyhedra are Eulerian*'. (It was Steiner's paper that provoked Hessel—the Lhuilier of the Germans—to write his [1832]).

TEACHER: I accept your example—but I did not know about Gergonne's proof. But if you did, why did you not tell us about it?

OMEGA: Because I immediately refuted it by non-Gergonnian polyhedra that were Eulerian.

GAMMA: As I have just said, I too found such polyhedra. But is that a reason for scrapping the proof altogether?

OMEGA: I think so.

TEACHER: Have you heard of Legendre's proof? Would you scrap that too?

OMEGA: I certainly would. It is still less satisfactory: its content is even poorer than Gergonne's proof. His thought-experiment started by mapping the polyhedron with a central projection on to a sphere containing the polyhedron. The radius of the sphere he chose as 1. He chose the centre of the projection so that the sphere will be covered completely, once but only once, by a network of spherical polygons. So his first lemma was that such a point exists. His second lemma was that for the polyhedral network on the sphere $V - E + F = 2$—but this he succeeded in decomposing into trivially true lemmas of spherical trigonometry. But a point from which such a central projection is possible exists only in convex and a few decent 'almost-convex' polyhedra—a class narrower even than that of 'quasi-convex' polyhedra. But this theorem: '*All Legendre-polyhedra are Eulerian*'[1] differs *completely* from that of Cauchy, but only for the worse. It is 'unfortunately incomplete'.[1] It is a 'vain effort which presupposes conditions on which the Euler theorem does not depend at all. It has to be scrapped and one has to look for more general principles'.[2]

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[1] *Legendre's proof* can be found in his [1794], but not the proof-generated *theorem*, since proof-analysis and theorem-formation were virtually unknown in the 18th century. Legendre first defines polyhedra as solids whose surface consists of polygonal faces (p. 161). Then he proves $V - E + F = 2$ *in general* (p. 228). But there is an exception-barring amendment in a note in fine print on p. 164, saying that only *convex* polyhedra will be considered. He ignored the almost convex fringe. Poinsot was first, in his [1809], to notice when commenting on Legendre's proof, that the Euler formula 'is valid not only for ordinary convex solids, namely, for those whose surface is cut by a straight line in no more than two points: it also holds for polyhedra with re-entrant angles, provided one can find a point in the interior of the solid which serves as the centre of a sphere on to which one can project the faces of the polyhedron by lines leading from the centre, so that the projected faces do not overlap. This applies to an infinity of polyhedra with re-entrant angles. In fact, Legendre's proof applies, as it stands, to all these additional polyhedra' (p. 46).

BETA: Omega is right. 'Convexity is to a certain extent accidental for Eulerianness. A convex polyhedron might be transformed, for example by a dent or by pushing in one or more of the vertices, into a non-convex polyhedron with the same configurational numbers. Euler's relation corresponds to something more fundamental than convexity.'[3] And you will never capture that by your 'almost' and 'quasi-' frills.

OMEGA: I thought Teacher had captured it in the topological principles of the Cauchy proof in which *all* the lemmas of Legendre's proof are replaced by completely new ones. But then I stumbled upon a polyhedron that refuted even this proof which is certainly the deepest hitherto.

TEACHER: Let us hear about it.

OMEGA: You all remember Gamma's 'urchin' (Fig. 7). That was of course non-Eulerian. But not all star-polyhedra are non-Eulerian!

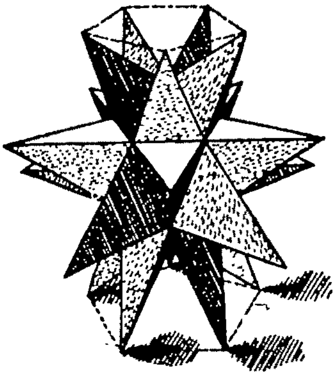

FIG. 15

Take for instance the 'great stellated dodecahedron', (Fig. 15). It consists, like the 'small stellated dodecahedron' of pentagrams, but differently arranged. It has 12 faces, 30 edges and 20 vertices, so that $V - E + F = 2$.[1]

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[1] E. de Jonquières goes on, again lifting an argument from Poinsot's [1858]: 'In invoking Legendre, and like high authorities, one only fosters a widely spread prejudice that has captured even some of the best intellects: that the domain of validity of the Euler theorem consists only of convex polyhedra' ([1890a], p. 111).

[2] This is from Poinsot ([1858], p. 70).

[3] D. M. Y. Sommerville ([1929], pp. 143-4)

TEACHER: Do you then reject our proof?

OMEGA: I do. The satisfactory proof has to explain the Eulerianness also of the ' great stellated dodecahedron '.

RHO: Why not admit that your ' great stellated dodecahedron ' is triangular? Your difficulties are imaginary.

DELTA: I agree. But they are imaginary for a different reason. I have taken to star-polyhedra now: they are fascinating. But they are, I am afraid, essentially different from ordinary polyhedra: therefore one cannot possibly conceive a proof that would explain the Eulerian character of, say, the cube, *and* of the ' great stellated dodecahedron ' by one single idea.

OMEGA: Why not? You have no imagination. Would you have insisted after Gergonne's and before Cauchy's proof that concave and convex polyhedra are essentially different: therefore one cannot possibly conceive of a proof that would explain the Eulerian character of convex and concave polyhedra by one single idea? Let me quote from Galileo's *Dialogues*:

> SAGREDO: So as you see, all planets and satellites—let us call them all ' planets '—are moving in ellipses.
>
> SALVIATI: I am afraid there are planets moving in parabolas. Look at this stone. I throw it away: it moves along a parabola.
>
> SIMPLICIO: But this stone is not a planet! These are two quite separate phenomena!
>
> SALVIATI: Of course this stone is a planet, only thrown with a less mighty hand than that one which launched the Moon.
>
> SIMPLICIO: Nonsense! How can you dare to pool under one head heavenly and earthly phenomena? One has nothing to do with the other! Of course both may be explained by proofs, but I surely expect the two explanations to be totally different! I cannot imagine a proof which should explain the course of a planet in heaven and a projectile on the earth by one single idea!
>
> SALVIATI: You cannot imagine it but I can devise it . . .[2]

TEACHER: Never mind projectiles and planets, Omega, have you succeeded in finding a proof to embrace both ordinary Eulerian polyhedra and Eulerian star-polyhedra?

---

[1] This ' great stellated dodecahedron ' has already been devised by Kepler ([1619], p. 53), then independently, by Poinsot ([1809]), who first tested if for Eulerianness. Fig. 15 is copied from Kepler's book.

[2] I was unable to trace this quotation.

Omega: I have not. But I shall.[1]

Lambda: Say you do—what is the matter with Cauchy's proof? You must explain why you reject one proof after the other.

(b) *Drive towards final proofs and corresponding sufficient and necessary conditions.*

Omega: You criticised proof-analyses for the breakdown of the *retransmission of* falsity by counterexamples of the *third* type. Now I criticise them for the breakdown of the *transmission* of falsity (or what amounts to the same, the *retransmission of truth*) by counterexamples of the *second* type. A proof must explain the phenomenon of Eulerianness in its entire range.

My quest is not only for *certainty* but also for *finality*. The theorem has to be certain—there must not be any counterexamples *within* its domain; but it has also to be *final*: there must not be any examples *outside* its domain. I want to draw a dividing line between examples and counterexamples, and not just between a safe domain of a few examples on the one hand and a mixed bag of examples and counterexamples on the other.

Lambda: Or, you want the conditions of the theorem to be not only sufficient, but also necessary!

Kappa: Let us imagine then, for the sake of the argument, that you found such a master-theorem: '*All master-polyhedra are Eulerian*'. Do you realise that this theorem will only be 'final' if the converse theorem: '*All Eulerian polyhedra are master-polyhedra*' is certain?

Omega: Of course.

Kappa: That is, if certainty gets lost in vicious infinity, so will finality? You will find at least one Eulerian polyhedron outside the domain of each of your ever deeper proofs.

Omega: Of course I know that I cannot solve the problem of finality without solving the problem of certainty. I am sure we shall solve both. We shall stop the infinite spate of counterexamples both of the first and the third types.

Teacher: Your search for increasing content is very important. But why not accept your second criterion of satisfactoriness—finality—as *mandatory* but not *obligatory*? Why reject interesting proofs that do not contain both sufficient and necessary conditions? Why regard them as refuted?

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[1] Cf. footnote 1, p. 244.

OMEGA: Well . . .[1]

LAMBDA: Whatever the case, Omega certainly convinced me that a single proof may not be enough for the critical improvement of a naive conjecture. Our method should include the radical version of his *Rule* 4, and then it should be called the method of '*proofs and refutations*' instead of '*proof and refutations*'.

MU: Excuse my butting in, I have just translated the results of your discussion into quasi-topological terms: The lemma-incorporating method yielded a contracting sequence of the nested *domains of successive improved theorems*; these domains shrank under the continued attack of global counterexamples in the course of the emergence of hidden lemmas and converged to a *limit*: let us call this limit the '*domain of the proof-analysis*'. If we apply the weaker version of *Rule* 4, this domain can be widened under the continued pressure of local counterexamples. This expanding sequence again will have a limit: I shall call it the '*domain of the proof*'. The discussion then has shown that even this limit domain may be too narrow (perhaps even empty). We may have to devise *deeper* proofs whose domains will form an *expanding* sequence, including more and more recalcitrant Eulerian polyhedra which were local counterexamples to previous proofs. These domains, themselves limit-domains, will converge to the double limit of the '*domain of the naive conjecture*'—which is after all the aim of the inquiry.

The topology of this heuristic space will be a problem for mathematical philosophy: will the sequences be infinite, will they converge at all, attain the limit, may the limit be the empty set?

EPSILON: I found a deeper proof than Cauchy's which explains also the Eulerianness of Omega's 'great stellated dodecahedron'! [*Passes a note to the Teacher.*]

OMEGA: The final proof! The true essence of Eulerianness will now be revealed!

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[1] The answer is in the celebrated Pappian heuristic of antiquity which applied only to the discovery of 'final', 'ultimate' truths, i.e. to theorems which contained both necessary and sufficient conditions. For 'problems to prove' the main rule of this heuristic was: 'If you have a conjecture, derive consequences from it. If you arrive at a consequence known to be false, the conjecture was false. If you arrive at a consequence known to be true, reverse the order and, if the conjecture can be thus derived from this true consequence, then it was true.' (Cf. Heath [1925], I, pp. 138–139.) The principle '*causa aequat effectu*' and the quest for theorems with necessary and sufficient conditions were both in this tradition. It was only in the seventeenth century—when all the efforts to apply Pappian heuristic to modern science had failed—that the quest for certainty came to prevail over the quest for finality.

TEACHER: I am sorry, time is running short: we shall have to discuss Epsilon's very sophisticated proof some other time.[1] All I do see is that it will not be final in Omega's sense. Yes, Beta?

(c) Different proofs yield different theorems

BETA: The most interesting point I have learned from this discussion is that different proofs of the same naive conjecture lead to quite different theorems. *The one Descartes-Euler conjecture is improved by each proof into a different theorem.* Our original proof yielded: '*All Cauchy-polyhedra are Eulerian.*' Now we have learned about two completely different theorems: '*All Gergonne-polyhedra are Eulerian*' and '*All Legendre-polyhedra are Eulerian*'. Three proofs, three theorems with one common ancestor.[2] The usual expression '*different proofs of the Euler theorem*' is then confusing, for it conceals the vital role of proofs in theorem-formation.[3]

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[1] The proof is Poincaré's (cf. his [1893] and [1900]).

[2] There are many other proofs of the Euler conjecture. For a detailed heuristic discussion of Euler's, Jordan's and Poincaré's proofs see Lakatos [1961].

[3] Poinsot, Lhuilier, Cauchy, Steiner, Crelle all thought that the different proofs prove the same theorem: the '*Euler-theorem*'. To quote a characteristic sentence from a standard textbook: 'The theorem stems from Euler, the first proof from Legendre, the second from Cauchy' (Crelle [1827], II, p. 671).

Poinsot came very near to noticing the difference when he observed that Legendre's proof applied to more than just ordinary convex polyhedra. (See footnote 1 on p. 239.) But when he then compared Legendre's proof with Euler's proof (that one which was based on cutting off pyramidal corners of the polyhedron and arriving at a final tetrahedron without changing the Euler-characteristic [1751]) he gave preference to Legendre's on the ground of 'simplicity'. 'Simplicity' stands here for the eighteenth-century idea of rigour: clarity in the thoughtexperiment. It did not occur to him to compare the two proofs for *content*: then Euler's proof would have turned out to be superior. (As a matter of fact, there is nothing wrong with Euler's proof. Legendre applied the subjective standard of contemporary rigour and neglected the objective one of content).

Lhuilier—in a surreptitious criticism of this passage (he does not mention Poinsot)—points out that Legendre's simplicity is only 'apparent', for it presumes considerable background knowledge in spherical trigonometry ([1812-13], p. 171). But Lhuilier too believes that Legendre '*proved the same theorem*' as Euler (ibid. p. 170).

Jacob Steiner joins him in the appraisal of Legendre's proof and in assuming that all proofs prove the same theorem ([1826]). The only difference is that while according to Steiner all the different proofs prove that '*all polyhedra are Eulerian*', according to Lhuilier all the different proofs prove that '*all polyhedra that have no tunnels, cavities and ringshaped faces are Eulerian*'.

Cauchy wrote his [1811] on polyhedra when he was in his early twenties, years before his revolution of rigour and one cannot take it amiss that he repeats Poinsot's

PI: The difference between the different proofs goes much deeper. Only the naive conjecture is about polyhedra. The theorems are about Cauchy-objects, Gergonnian objects, Legendrian objects respectively, but not any more about polyhedra.

BETA: Are you trying to be funny?

PI: No, I shall explain my point. But I would do this in a wider context—I want to discuss *concept-formation* in general.

ZETA: We should rather first discuss *content*. I found Omega's *Rule* 4 very weak—even in his radical interpretation.[2]

TEACHER: Right. Let us then first hear Beta's approach to the problem of content and then wind up our debate with a discussion of concept-formation.

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comparison of Euler's and Legendre's proofs in the introduction to the second part of his treatise. He—like most of his contemporaries—did not grasp the difference in depth of different proofs and so could not appreciate the real power of his own proof. He thought he had just given yet *another proof of the very same theorem*—but he was rather eager to stress that he had arrived at a rather trivial generalisation of the Euler-formula to certain aggregates of polyhedra.

Gergonne was the first to appreciate the unrivalled depth of Cauchy's proof (Lhuilier [1812-13], p. 179).

[2] See p. 237.

(*To be continued*)

REVIEWS

THE AIMS AND INTERESTS OF SOCIOLOGY

1 *Introduction*

In recent years the problems of change and development have come to occupy a central place in sociological discussion. A whole generation of younger sociologists, interested in the large-scale social changes that are taking place in the underdeveloped countries, are turning once again to the ideas of their nineteenth-century predecessors. But they do so knowing that some of these ideas can only be revived at the risk of destroying the new foundations upon which contemporary sociology has been built: these ideas, or doctrines, are those which K. R. Popper has denounced as historicism.

The Poverty of Historicism,[1] by Karl Popper, is one of the few important books on the methodology of the social sciences to appear in this or any other century; in the refinement of its logical analysis it is probably the most subtle; but it is also one of the most controversial; for it not only attacks the most vulnerable of historicist ideas, but aims at demolishing a whole system of ideas which are alleged to derive from historicist assumptions; and it also prescribes the legitimate aims which sociologists should embrace. Some sociologists, at least in Britain, resent this; they are sufficiently doubtful about the 'scientificity' of sociology and its legitimate preoccupations to be easily incensed by one who questions those tenets which they have been least successful in defending. But this is not the only reason for their reluctance to accept Popper's strictures and prescriptions: there are others, and I hope to show what they are.

The book itself is divided into four parts: in the first two Popper presents the main elements of two forms of historicism, the anti-naturalist and the pro-naturalist; in the third and fourth part he presents his own criticisms of each form. There are four main themes in the book: the defects of historicism; the moral and political implications of historicism; proposals for a non-historicist sociology; and proposals for a non-historicist moral and political policy. Only the first and third of these themes—those concerned primarily with the methodology of the social sciences—will be considered here.

2 *The Defects of Historicism*

According to Popper, the central aim of historicism is this: to discover the laws which govern the inevitable succession of historical stages, and to use this discovery to predict future stages.[2]

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[1] K. R. Popper, *The Poverty of Historicism*, Routledge & Kegan Paul, London, 1961, paperback, pp. x + 169, 6s.

[2] See ibid., p. 3

There are two forms of historicism: the anti-naturalist, which opposes the methods of the social sciences to those of the natural sciences; and the pro-naturalist, which asserts the unity of method of all sciences.

The anti-naturalist doctrine rests on a number of assumptions: all historical phenomena, including social systems, are unique in their characteristics; these unique configurations can only be comprehended intuitively as wholes; the only regularity in social life is the law governing the stages of historical succession.

The pro-naturalist doctrine rests on different assumptions: the method of natural science is to discover the laws governing the causal sequences of past events and to predict future ones; the method of sociology is, therefore, to discover the laws governing the sequence of historical events and to predict future ones; this is also achieved by discovering the laws governing the transformation of each system. Some of the pro-naturalist doctrines anticipated Darwinism, while others were later inspired by it.

Popper attacks these assumptions by arguing: that the methods of natural science *are* applicable to the study of society; that they have nothing in common with those of historicism; that the existence of unique characteristics does not preclude the existence of general ones; that historicism rests on another false doctrine, that of 'holism'; that the findings of Darwin give no support to historicism; and that historicism is a logically impossible method, and that its assertions can be refuted.[1]

Popper's criticisms of historicism seek to show, in the main, that a science can be either theoretical or historical, but not both simultaneously. A theoretical (empirical) science is one in which the main focus of interest is the search for universal empirical propositions which state invariant causal relations, and which assert the impossibility of certain occurrences in the real world. An historical science is one in which the main focus of interest is on the occurrence of particular events in causal sequences, and on the uniqueness or particularity of converging chains of causal sequences. The hypotheses of historical sciences are not universal propositions but singular ones.

The relationship between theoretical and historical foci of interest is as follows: In theoretical sciences universal propositions (major premisses) are tested by deducing from them statements which assert the occurrence of some event (conclusions); this can only be done with the aid of other statements (minor premisses) which state the initial conditions under which the event actually occurs; the hypotheses, whose truth is not assumed, are universal propositions (major premisses). In historical sciences, on the contrary, the truth of the statements of initial conditions (minor premisses) can not be assumed, and must be tested. In both theoretical and historical sciences use is made of universal and singular statements, but the focus of interest differs from one to the other.

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[1] Popper himself distinguishes between his criticisms of historicism and his refutation of it; the actual refutation does not appear in the body of the book but in the preface (pp. v–viii).

Historicism, Popper argues, seeks to ignore this difference by asserting laws or universal propositions concerning the *actual sequences of historical events;* but there can be no such laws, since each event, or historical ' stage ' is produced by a *number* of causal sequences whose convergence at any particular time is, to a considerable extent, accidental, *in that it is not determined by any particular law or laws.* In fact, says Popper, ' no sequence of, say, three or more causally connected concrete events proceeds according to any single law '.[1]

This is not to deny that there can be laws concerning social phenomena: there can be and indeed are, such as those of economics. But they are not laws which assert a past series of events from which future ones can be predicted: indeed, statements of past ' trends ' cannot be used predictively unless they are themselves explained by universal statements which state the conditions under which such trends occur. For example, one cannot predict a future trend of capital accumulation and concentration on the basis of a past trend of this kind: but one can make a *conditional* prediction of this kind, if the *conditions* under which the trend occurs are assumed to obtain in the future.

Historicists, like Marx and Comte, consistently confused statements about trends with law-statements proper, and therefore sought to justify historicist sociology in terms of criteria which simply do not apply to the natural sciences: they make claims for historicism which are greater than those which can be made for physics!

The attempts to save historicism by appealing to the success of the evolutionist approach in biology are equally misconceived. For the laws of evolution are not statements about the actual stages of the evolutionary process, but are about the *mechanisms* of evolution. The statements about stages are historical hypotheses which assert the occurrence and convergence of particular events; though the statements about natural selection, genetic transmission and mutation *are* genuine theoretical hypotheses.

The final defect in historicism is its logical impossibility. Predicting future states of society requires a knowledge of future conditions which will affect that future state; one of these conditions is the future state of knowledge itself; but future knowledge cannot be predicted—it is in the very nature of the growth of knowledge that it cannot be predicted, else it would be part of present knowledge, and the task of predicting its future state would still exist—consequently, future social states cannot be predicted in so far as they are affected by future knowledge.

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[1] *The Poverty of Historicism*, p. 117

3 *Prescriptions for a Scientific Sociology*

Popper is convinced that the defects of sociology are due to its historicist ancestry.[1] He therefore feels bound to remedy these defects and to propose a programme for a 'genuine' theoretical sociology. His proposals are: that sociologists should seek to formulate non-historicist universal propositions and to test them; that they should define their interests in terms of practical social problems; that they should adopt the method of individualism; and that they should provide models and theories for the interpretation of history.

One of the major assertions of the anti-naturalist historicists is that there can be no universal statements about social phenomena other than those governing the law of 'historical motion', since all social systems are unique 'wholes', the product of a unique historical process.

Popper rebuts this argument by showing that concentration on the unique can blind one to the existence of general characteristics and their causes. Each earthquake is unique, and a detailed description of one will differ from a detailed description of another: however, there are still natural laws which govern the processes which occur in earthquakes. Similarly, the fact that social systems are unique historical products does not prevent a sociologist from investigating universal social processes. No two revolutions are the same, but this does not detract from the truth of the statement 'all revolutions are followed by reactions'. No two structures of power are identical, but the truth of the statement 'all power corrupts' is not thereby weakened.

Popper wishes sociologists to focus their attention on the truth or falsity of statements of this very kind—for example, those asserting the corrupting influence of power, or the partial failure of revolutions—and he thinks that only by doing so will they justify their right to existence.

He does not deny that there are special problems in the testing of these universal hypotheses, but thinks that these have been exaggerated unduly: thus, statistical techniques can be substituted for controlled experiment; and the observation of social experiments—that is, social reforms—can be substituted for the creation of laboratory ones. But he readily admits that there may be some difficulty in constructing rigorous tests if the strength of variables cannot be weighted.[2]

By focusing their attention on practical problems—by adopting what Popper calls a 'technological' approach—sociologists will be encouraged to formulate genuine testable theories. For, the search for solutions to practical problems—such as crime, administrative inefficiency, the establishment of democratic institutions, etc.—leads to an investigation of their necessary or sufficient conditions. This activity will serve to keep sociological feet on the ground rather than in the air of metaphysical speculation, which is characteristic of historicism and of much else that passes for sociology.

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[1] 'Since I am convinced that such historical doctrines . . . are at bottom responsible for the unsatisfactory state of the theoretical social sciences (other than economic theory) . . .' (ibid., p. 3).

[2] Ibid., p. 142

Popper's plea for the adoption of the individualistic approach—or, of 'methodological individualism', as he calls it—arises out of his critique of the 'holistic essentialist' element in historicist thinking. For historicists, social phenomena are real entities, whose 'essence' is expressed in the historical process; for Popper, social phenomena are abstractions—or 'models'—the real entities being individuals and their interactions; for him the models are only of use in analysing social processes in terms of the *unintended consequences of individual interactions.*

Popper distinguishes two doctrines of 'holism'. The first asserts that social structures are real entities which must be comprehended, intuitively, as wholes, in order that their place in the historical process be explained. The second simply asserts that the whole is more than the sum of its parts, and that the pattern of such wholes can be abstracted from their concrete context.

Popper rejects the first doctrine on the grounds that it is logically impossible to comprehend the whole of anything, whether intuitively, or any other way: for the totality of anything is infinitely complex; any comprehension of an object is partial, and the observation of it is always selectively perceived in terms of some problem or theory. He accepts the second doctrine but finds it trivial and of little special relevance for sociology: for, he argues, anything in the world can be said to exhibit a pattern of interrelationships which is more than the mere sum of its parts.

Popper's final prescription for sociology arises out of his distinction between schemes of interpretation and genuine theories. It is his contention that the so-called 'theories' of history are merely interpretations. While a genuine theory asserts the impossibility of something—thus, a theory which asserts that purely objective competitive examinations are a necessary condition for the existence of a completely efficient bureaucracy, does not assert anything about the actual existence of either phenomenon, but merely states that one is not possible without the other—an interpretation does actually state that something occurs, and does so by deliberately emphasising certain aspects of concrete occurrences and relating them to others. The so-called 'law of evolution' which asserts the stages of evolution and the tendency towards an increased complexity and diversity of living forms, is not a law at all; it is an interpretation of a large, but finite number of facts. A genuine law—that is, a well tested universal proposition asserting causal connections—refers not to a finite number of occurrences, but to an infinite number. 'Theories' of history are interpretations guided by schemes which determine the choice of facts which are interrelated: they can not be strictly tested—that is, they do not prescribe the events whose occurrence would lead to their refutation—since they are formulated in such a way as to exclude those occurrences which do not accord with them.

Popper suggests that historical interpretation could be advanced, in dealing with institutional changes and mass movements, etc., by the use of models provided by sociology. He says little or nothing of what these models would look like, but one might assume that he has in mind the models of economic theory.

4 *Discussion of Popper's Critique of Historicism*

Popper's critique of the central doctrine of historicism is unassailable: there can be no law determining either the stages of social development or the cyclical 'movement' of societies. Societies are not objects and to say that they move is, in any case, a metaphor to describe the fact that some features of social life do change.

But there are two things about Popper's discussion of historicism which are disquieting. In the first place, he seems to condemn, under the term historicism, any sociological interest in the broad development of human society.[1] Secondly, while he claims to erect arguments to strengthen the historicist case,[2] thereby claiming the maximum for his critique, he fails to show much sympathetic interest in the problems which preoccupied historicists, and which still preoccupy some sociologists now; and therefore fails to recognise alternatives to the historicist method in dealing with these very problems.

Some of these problems can be defined as follows: What has been the course of development of social structures? To what extent has there been a development from simpler to more complex forms? What are the limits within which social structures of certain kinds can change, and to what extent have these limitations resulted in a particular line of development in human history? What are the dominant structural characteristics of societies of different levels of complexity, and how do these dominant characteristics affect other areas of social life, such as the degree of freedom accorded to the individual, the forms of justice and morality which obtain? To what extent does an increase in complexity of social structure produce an increasing degree of autonomy of different areas of social life? Does an increase in the degree of structural autonomy result in an increasing tendency for social structures to change? And so on.

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[1] 'Considering that Mill's "States of Society" correspond precisely to what we have called "historical periods", it is clear that his distinction between the "two kinds of sociological enquiry" corresponds to our distinction between the approach of piecemeal technology and that of historicism' (p. 71). But a concern with 'states' or 'stages' of social development need not be historicist at all in the sense that it presupposes a law of succession of stages.

[2] Ibid., p. 3

The method of dealing with these problems need not be that of searching for a single law of development. It can be that of constructing models of societies of different levels of complexity in an attempt to isolate certain dominant factors and to analyse their interrelationships. For it is clear, that at various levels, the possibilities of change are indeed limited, though this is more so at the simpler levels than at the more complex.

Now this approach is not as remote from the aims of some historicists as it may seem; for although historicists have asserted an *overall* law of social development, they have also asserted that the particular conditions of change of each system were to be found within it. It is true that they tended to neglect exogenous factors, both natural and social, and that they also overemphasised the degree to which the internal factors were interrelated and ' bounded ' as a system, thereby *confusing their models of social systems and their developments with the realities of social life and history.* In this they were mistaken. But their mistake was not, as Popper would insist, a fatal one; for one can indeed learn from it, and still try to analyse the processes of change and development—even construct schemes of social development—in order to answer some of the very questions raised by them. The aim would not be to discover the law of succession of stages, but the particular conditions of them; though it is possible that these particular conditions can be formulated as models to apply to a number of different cases where the type of development is similar.

But the very use of terms like ' stages ' seems to arouse Popper's suspicion.[1] For he is convinced that historicists could only conceive of these as completely divisible entities. But this is to take them too literally, and to ignore their own qualifications in using them. Indeed, the concept of a ' stage ' can be interpreted quite reasonably to refer to the dominant characteristics of a society at a particular time in its history, which it may well share with other societies, whether coeval or not.

Popper also seems to suspect the very term ' historical method ', when used in the discussion of social structures.[2] But why should he equate this with historicism? The idea that social institutions can only be analysed historically may, in some cases, be inapplicable, and in others inappropriate. But it is not necessarily a part of historicism. In fact, some members of the German historical school, who also used the concept of ' stages ' and later of ' ideal types ', denied that historicists, like Hegel, did in fact use an historical method at all; it was they who emphasised the uniqueness of social systems and of the processes whereby they came into existence.

Popper is also unfair when dealing with the problem of uniqueness. For it is not a question of whether there are also universal causal connections which can be discovered in a variety of periods or social systems—no doubt there are—but of whether it is more interesting or relevant to concentrate on these rather than on the social systems as such. For it is often the case in explaining or analysing a social phenomenon, whether synchronically or diachronically,[1] that the regularities of a social system are the factors which enter into the explanation. These regularities are usually particular factors —initial conditions, in Popper's sense—and reference to them still entails the use of universal propositions; but the latter are often trivial and vague in content, and their truth can be assumed, while the statements about the former are the things to be investigated. The particular combination of factors that characterise a social system *does* have consequences that are different from those that result from another combination of factors, even though many of the factors are common to both systems; in some cases a sociologist may have to treat his model of a particular system as if it were a 'theory' of that system. Strictly speaking, there cannot be a genuine theory for each system; and these models are not theories in any strict sense of that term; *but they perform some of the functions of genuine theories.*[2]

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[1] Mill's ' States of Society ', p. 71

[2] Loc. cit.

5 *Appraisal of Popper's Proposals for Sociology*

Popper's general argument that there are universal empirical propositions for sociologists to find (if only they would look for them) is sound. No doubt there are such propositions. But sociologists may fear that many will, if found, turn out to be trivial if true, and false if significant. Surely, two of the most important criteria for the evaluation of a theoretical hypothesis in any science are that it should have content, and that it should be expected to stand up to some rigorous tests, at least for a while. If it is suspected that it will either have little significant content or that it will be immediately refuted by some empirical case, there is little incentive to treat its discovery as the ultimate goal. It is this, rather than historicist influence, which deters some sociologists from seeking laws.[3]

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[1] The term 'synchronic' refers to the analysis of a social structure as given and unchanging at a particular point in time; the term 'diachronic' refers to an analysis of structural changes which occur between two points in time. (See A. R. Radcliffe-Brown, *Structure and Function in Primitive Society*, Illinois, 1952, p. 4.)

[2] Popper is understandably and justifiably suspicious of 'holistic' terms. The danger of reifying such concepts as 'social system', and of attributing 'self-propelling' qualities to them, is real. But there are legitimate ways of using these terms and even of attributing 'self-propelling' qualities to social systems, provided one realises that one is using an artificially insulated model. Popper would surely not object to an economist who claimed that one economy was likely to grow more rapidly than another. Why should he object to a sociologist who claimed that one social system was likely to produce a more rapid increase in individual freedom than another?

[3] See, for example, E. E. Evans-Pritchard, 'Social Anthropology: Past and Present', in *Essays in Social Anthropology*, London, 1962, p. 20. He says: 'What general statements have been made are . . . too general to be of value. Often they are . . . guesses on a . . . *post factum* level, and they sometimes degenerate into mere tautologies . . .' It should be noted that the author has no sympathy with historicism, as defined by Popper, but does subscribe to the views of the 'historical school'.

There is, indeed, much justification for this view. Very few universal propositions stating the causal connection between factors of social structure survive empirical tests; and propositions at a higher level of abstraction are either similarly falsified or rendered trivial by attempts to 'save' them. It may be that the future holds brighter prospects for sociology, but to date there is little cause for optimism for those who believe that it should be a predominantly theoretical science, in Popper's sense.

However, this is no reason for sociologists to abandon their interest in theoretical statements: for even if these are no sooner propounded than refuted, the refutations do advance knowledge; and the theoretical statements, such as they are, do perform what Popper has called an important 'searchlight' function for the analysis of concrete situations. Thus, the statement 'bureaucracy is incompatible with a system of agnatic lineages', is of value even though it can be refuted empirically.[1] Its value lies in guiding the analysis of situations where bureaucracy is introduced into a society with lineage organisation, by directing attention to those *additional* factors which may make for the compatibility of the two institutions.

Some sociological theories really are little or nothing more than programme statements while others do approximate to 'genuine' theories, and it is possible that both, particularly the latter, could be formulated in more rigorous form so as to become genuine empirical theories; but even if this were not so, they would not thereby lose their usefulness or even their indispensability.

An example of a 'programme' theory is Max Weber's assertion that there is a tendency for 'charisma' to become 'routinised'. This is not a strictly testable universal proposition, since it says little or nothing about degrees of 'routinisation', the length of time which elapses before 'routinisation', occurs or becomes dominant, etc. But these more precise conditions and characteristics of the process are dependent on a large number of institutional, transient-situational, and other factors, which can be present in various combinations and forms with different consequences; indeed, so numerous are the possible combinations, as to make attempts at precise universal statements both self-defeating and worthless. Yet despite the loose and, in Popper's terms, metaphysical[2] nature of this proposition, it is an extremely valuable contribution to sociology, as is Weber's explanation of why the process should occur. Such contributions are, in themselves, of some intrinsic interest, but their major use is in directing investigation into the similarities and differences between different historical and contemporary processes of this kind; such research does not—possibly will not—produce tested universal propositions; but it does produce a deeper understanding of each case, and of some characteristics which types of case have in common; it produces hypotheses about *closed*[1] classes of phenomena.

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[1] See M. Freedman, *Lineage Organisation in Southeastern China*, London, 1958, particularly pp. 114 and 139.

[2] Metaphysical in the sense that it is not simply refuted by the occurrence of counter-examples; it is not metaphysical in the non-Popperian sense of referring to non-existent entities; charismatic leaders, unlike unicorns, do exist.

There are indeed cases where the pursuit of universal propositions is itself fruitful. But even in these cases, the really interesting findings may be in the application of the theory to the analysis of different societies. An example, again, will illustrate this. There are broad similarities in the nature of crime in different industrial societies and these suggest the likelihood that there are similar causes. In fact the causal connections have been stated in a number of theories as follows: In all industrial societies there is a marked emphasis on personal achievement and success; the legitimate means for attaining success are scarce; there are 'underprivileged' groups whose status encourages the growth of norms which are accepted as legitimate by members of these groups, but which are opposed to the ideal norms of the wider society; there are always a number of individuals whose moral education is deficient and over whom the constraints of authority are inadequate, either because of their psychopathological tendencies or because of their social situation (e.g. immigration); the interaction of these social situational and internal psychological factors produces criminal behaviour.

These theories are by no means intrinsically uninteresting, and are worthy of being rigorously tested. But they do not in themselves go very far in explaining the *particular pattern of crime* which emerges in any particular industrial society (and such patterns do vary) though they do *suggest* the kinds of factors which should be sought in each case. In the final instance the criminologist must use a model of each society, together with the general theories of criminal behaviour, and treat each model as a kind of 'theory' of each society.

One can also quote an opposite instance to the one just discussed, in which a discussion of one society has led to an interest in universal propositions. This is the oft-quoted case of the Protestant ethic. Weber suggested that the development of capitalism in the West was, in large part, due to the independent, and prior development of Calvinist Protestantism. It was his intention also to show that the absence of a similar religious ethic was in part responsible for the non-appearance of capitalism as a spontaneous development in such societies as India and China. The idea has since been formulated that, in the absence of some social and cultural conditions which promote an 'achievement orientation',[2] industrialisation as such is inhibited, the motive for personal achievement being characteristic of any industrial society, whether capitalist or not. The argument has been shifted from the particular plane of Protestantism and Capitalism to that of an 'achievement ethic' and industrialism. But even here, the focus on a theory is less significant than it may appear. This is so for two reasons: the new theory, by emphasising the more general class of conditions, namely an 'achievement ethic', as necessary for industrialism (and hence for industrial capitalism) says nothing about those peculiar characteristics of the Protestant ethic which Weber was at such pains to emphasise; the second reason is that the achievement ethic need not be a temporal pre-condition of industrialisation, but may be a consequence of an early stage of industrialisation which has been introduced forcibly, or by an external agent, prior to the emergence of the 'required' attitudes, values, and motives. The real significance of the theory lies in the guidance which it gives in the analysis of social structures undergoing industrialisation, some of which exhibit greater obstacles than others.

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[1] In so far as propositions are about 'closed' classes of phenomena they are not universal, and are therefore not theories; they assert actual occurrences.

[2] This view has been put forward by D. C. McClelland, *The Achieving Society*, Princeton, 1961.

But why is Popper so insistent that sociology can or should be a theoretical discipline? Is it because he is so impressed by the deductive schemes of economics (whose theories are so seldom tested by empirical observation)?

Popper himself insists that the major task of science is to explain: if historians can explain—that is, give answers to questions—without focusing much attention on the truth or falsity of their universal assumptions, why can sociologists not do the same, by focusing their attention on the specific structural and cultural factors which obtain in a particular society or type of society? Why must sociologists treat their universal assumptions as the main focus of their interest, particularly when they are faced with so much difficulty in subjecting them to rigorous tests? (Popper himself concedes that in the social sciences it is often very difficult if not impossible to give precise weights to the factors considered, so that prediction, which is nothing else than the method of testing universal propositions, becomes impossible.[1]) The answer could be that unless the universal assumptions can be stated rigorously (so that they render the kind of prediction which can be treated as a refutation of the theory), the explanations of sociology (and history) will remain partial ones, and that sociology will be of no real 'technological' use.

I think there is also another reason for Popper's insistence. He wishes to characterise the difference between history and sociology in purely logical terms by distinguishing between singular and universal hypotheses. But there *is* another possibility: namely, to distinguish between statements about structure and statements about actions as particular events. Sociologists are interested in the structured features of human social life, and treat actions, as particular events, as evidence to test structural hypotheses;

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[1] *The Achieving Society*, p. 142

historians, however, are more often interested in the actions as events themselves, though they may use structural hypotheses in pursuing this interest. Admittedly, not all historians are interested in Cleopatra's nose; but some might be. No sociologist is interested in Cleopatra's nose, but some of them might be interested in the Roman interest in noses, and might even compare this interest with similar interests in other societies.

It would be absurd to insist that *all* structural hypotheses are about 'closed' classes, and are therefore singular or particular propositions. They can be either singular or universal. It would be equally absurd and false to insist that no sociologist ever does or ever should focus his interest on the testing of universal hypotheses. But it would be no less absurd to legislate that sociologists commit themselves to being social physicists, or that they ape the economists in all respects.[1]

Popper's other major proposal for sociology is that it should be based on 'methodological individualism'. Much has been written for [2] and against [3] this doctrine and I do not propose to enter the lists as a combatant, largely because I think that the disputants have been arguing at cross purposes.[4] The trouble with Popper's own formulation of the doctrine is that it is so vague and ambiguous. He says that social phenomena should be analysed in terms of the actions and interactions of (usually anonymous) individuals. Does Popper mean that the observations of individual actions and interactions can be pieced together to provide a description of social phenomena? If he does, then he is guilty of crass empiricism, of the kind which he himself is at such pains to attack. For the observation of individual actions and interactions can only be understood if these are interpreted in the light of a conception of social structure: for example, the reciprocal behaviour of younger men to older and of older to younger can only be understood if the terms ' older ' and ' younger ' refer to reciprocal social roles which are interrelated with a number of other roles which together form a coherent system. But it is almost certain that Popper would agree; so he must mean something else.

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[1] The question as to why economics is, and has for long been the most successful of the social sciences, is a very interesting one, and cannot simply be answered by pointing to the historicist ancestry of sociology. After all, why did historicism succeed as it did, without seriously obstructing the development of economics? Mill was a non-historicist in economics, but, according to Popper, an historicist in sociology. The answer, I think, has much to do with the nature of economic systems in complex, highly differentiated societies. But this is not the place to elaborate this point.

[2] For an early formulation of this doctrine see F. A. Hayek, *The Counter-Revolution of Science*, Illinois, 1952, 36-43. For examples of 'Popperian' exegesis, see J. Agassi, 'Methodological Individualism', *British Journal of Sociology*, 1960, **3**, 244-270 ; I. C. Jarvie, 'Nadel on the Aims and Methods of Social Anthropology', this *Journal*, 1961, **45**, especially pp. 18-19; J. W. N. Watkins, 'Ideal Types and Historical Explanation', this *Journal*, 1952, **3**, 22-43, and 'Historical Explanation in the Social Sciences' this *Journal*, 1957, 104-117.

[3] See E. A. Gellner, 'Explanation in History', *Aristotelian Society Supplement*, 1956, **30**, 157-176; M. Ginsberg, 'The Individual and Society' in the volume, *On the Diversity of Morals*, London, 1957; L. J. Goldstein, 'The Two Theses of Methodological Individualism', this *Journal*, 1958, **33**, 1-11; M. Mandelbaum, 'Societal Facts', *British Journal of Sociology*, 1955, **6**, 305-317, and 'Societal Laws', this *Journal*, 1957, **31**, 211-224.

[4] This has been noted in a remarkable clarification of the whole problem by K. J. Scott, 'Methodological and Epistemological Individualism', this *Journal*, 1961, **44**.

Does he mean that all social action can ultimately be reduced to individual dispositions which are themselves independent of social arrangements? But if he does, then he would be guilty of psychological reductionism; and he himself denies that he subscribes to this doctrine. In fact, he goes further, and asserts that the social sciences are relatively independent of psychology and are more concerned with the logic of situations than with the psychology of motivations.[1]

Does he then mean that the principal aim of the social sciences is to discover non-psychological laws of individual action and interaction, from which statements about social institutions, etc., can be deduced? If he does, then he is asking the impossible. For it is in the nature of sociological statements of a high degree of abstraction and generality that they are vague and imprecise: they are either statements with very general predicates, or else they are not strictly universal propositions at all—that is, they do not begin with the words ' All ' or ' Whenever ', but with the words ' Many ' or ' Often '. This is so for a number of reasons. First, it is difficult or impossible to state the precise conditions under which they hold. Second, some of the conditions are highly subjective ones, and it is in the nature of these that their characteristics can often only be known *post hoc.* In fact, to know the subjective conditions—that is, the factors which determine the way in which individuals interpret their situations—is usually to know something about a particular cultural system. Thus, a law of universal competition might state: ' whenever two or more individuals seek the same thing, each will try to outwit or remove the other(s) in order to achieve his aim.' But this statement is known to be false. For, sometimes, when two or more people compete in this way one will join with another against a third and later divide the desired object, provided the object is divisible; at other times, one contestant will give way to another because of the latter's superior status. The law only holds under those conditions where nothing disturbs each partner's commitment to the goal in question: but this would hardly render a statement from which one could deduce very much about human social life; though such a statement will be used in any attempt to explain, in principle, rather than in detail, certain forms of competition in certain structural and cultural conditions.

In fact, it can be shown that the major contributions to sociology do not take the form of statements of such high levels of generality. It may be the case in physics that the science advances by extending the 'umbrella' of explanation: that is, by seeking to explain one univeral proposition by another which is not only more general but also more, or as precise as, the one to be explained.[1] But it is not the case in the social sciences, not even in economics.[2] Here the contributions are propositions about the structural characteristics of societies, or they are those which immediately explain such characteristics. But they are not usually those which refer to the nature of individual actions and interactions in the abstract. In fact, it might be said that what are surprising or novel about social science discoveries are these former propositions, and that what are trivial and uninteresting are the latter.[3]

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[1] This *Journal*, 1961, 44, pp. 157-158

It is doubtful whether Popper *is* suggesting that sociology should be primarily concerned with the laws of individual action and interaction, since his own examples of testable universal hypotheses are not of this kind.[4] It seems that what he is suggesting is this: sociological theories should not rest on the assumption that societies, institutions, social structures, etc., have aims, intentions and purposes, but on the assumption that only individuals have these; and that it is the unintended consequences of purposive actions which produce social structures.[5]

This view is, in the main, unexceptionable; but it has been interpreted in such a way as to deny the possibility of explaining social phenomena in terms of their *functions*—that is, in terms of their consequences for other social phenomena—on the grounds that this would amount to an attribution of aims and purposes to social phenomena.[6] This objection is, on the face of it, reasonable; for it would seem teleological, and therefore impermissible, to call a consequence a cause, and its preceding conditions its effect. But it really depends upon what is being explained. One certainly cannot explain the origin of an institution in terms of its later consequences, even when it has been deliberately designed with these consequences in mind; for the cause—or one of the causes—is the intention of the planner who successfully predicts the outcome, not the outcome itself. But while it is impermissible to explain the origin of an institution in terms of its consequences, whether intended or unintended, it is possible to explain its *persistence* in terms of its consequences, even where these latter are unintended. For an institution may persist simply because it has consequences which provide favourable conditions for its persistence. Thus, certain religious institutions have the consequence of maintaining and even promoting the solidarity of the groups who practice its rituals and share its beliefs; this solidarity may have the consequence of strengthening or maintaining the adherence of the group members to the particular religious cult; in this case, to explain the persistence of the religious cult by referring to its consequences is quite permissible; and this, without attributing aims and intentions to the religious institution, to the social structure or even to the members of the group.

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[1] See K. R. Popper, *The Logic of Scientific Discovery*, London, 1959, pp. 276-277

[2] Thus, the discovery of the law of 'diminishing marginal utility' (or 'diminishing marginal rate of substitutability') is not an end in itself, whereas the discovery of the laws governing the level of employment or prices is. It should be noted that the former is a higher-level proposition than the latter, but that it states, in effect, what is well known, while the latter does not.

[3] This view has been elaborated by F. A. Hayek. See 'Degrees of Explanation', this *Journal*, 1955, **23**, especially pp. 212-213.

[4] Ibid. p. 62

[5] While Popper notes that social systems are not entities, and cannot therefore have purposes, he seems not to realise that individuals are also, in a sense, abstractions. Of course, individual *organisms* are entities, but individual *persons* are not; they are a complex of social roles and personality traits; and it is in the former alone that sociology is interested; thus the individual—the performer of social roles—is, for the sociologist, an abstraction.

[6] See J. Agassi, 'Methodological Individualism', *British Journal of Sociology*, 1960, **3**, 266

Unfortunately, some functional propositions in sociology do appear to go further than this in suggesting that social phenomena arise because they are 'required' by a social system; as though the social system did, in itself, have aims. This kind of statement is usually the product of intellectual laziness, though sometimes of 'holistic' absurdity. If it has any acceptable meaning it is that, in some circumstances, institutions are unlikely to survive if they are incompatible with those which already exist; and that, if they do, the others will have to change. It could also mean that some, possibly unintended, social changes, do make the members of a society aware of the need for certain other changes; or that some changes have unintended consequences which provide more suitable conditions for their continuance. Whatever the reasonable meaning given to such statements, none need imply the existence of societal aims or intentions.

The dangers of organismic thinking have long been known to some sociologists, particularly through the writings of Max Weber. However, not all historicists subscribed to the organismic theory of society; certainly not Marx. And some of those who have recently appeared to use an organismic model, or something like it, have had no sympathy for historicism.[1]

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[1] Compare the following statements by a renowned American sociologist:
'The significance of the concept of function implies the conception of the empirical system as a "going concern".' (T. Parsons, 'Position and Prospects of Systematic Theory in Sociology', in *Essays in Sociological Theory*, Revised Edition, Illinois, 1954, p. 217)
'Functional significance in this context is inherently teleological.' (Loc cit.)
'On a relatively complete and explicit level this type of generalised system has been most fully developed in physiology . . .' (Ibid., p. 218)
'On this level no competent modern sociologist can be a Comtean, a Spencerian, or even a Marxian.' (Ibid., p. 220.) The reasons Parsons gives for this are that philosophies of history are not adequate sociological theories, and that they in any case have a 'notorious tendency to overreach the facts'. (Loc. cit.)

6 *Conclusions*

Karl Popper sets out to achieve two main things in this book: to attack and undermine the logical (or illogical) foundations of historicism; to set sociology to rights. In his former role he has produced a *tour de force;* in his latter he has uttered some important warnings.

Popper's conviction—from him, the term 'hypothesis' would have been more advisable—that the evils which beset sociology are due to its historicist ancestry, is like many convictions, part truth part dogma. The major developments in sociology since the death of Max Weber have occurred in America, and if any one thing is characteristic of American sociology it is its remoteness from historicism (and possibly from history itself).

It is also doubtful whether sociology is in as sorry a state as Popper seems to think. That it abounds in trivialities, that some of its high-flown systems are pretentious and useless in the extreme, and that it suffers from a lack of continuity in research activity, cannot be denied; nor, however, can its remarkable achievements; and one cannot blame all of its faults on historicism, of which much is both interesting and a stimulus to more reasonably defined goals and more adequate and more permissible methods of enquiry.

The defects of historicism have been known for some time; they were certainly known to Durkheim,[1] and of course to Max Weber, But, before Popper's attempt, no one had exposed the main logical errors of historicism or analysed the faulty suppositions which underly it. Furthermore, no one has discussed these problems with more clarity, though many have succeeded in obscuring the issues even more than the most metaphysical of historicists. In fact, for his clarity and logical ingenuity alone, Popper deserves to be read.

There are two main lessons to be learned from this book: the first, to apply the correct methods of science to all sociological enquiry, regardless of whether the achievements of the social sciences can ever match those of the natural sciences, on the grounds that non-scientific methods will yield nothing at all; the second, to be interesting—Popper always is.

It is perhaps all the more unfortunate that, considering its merits, so important a critique of sociological methods does not exhibit a greater familiarity with more recent developments in the aims, interests and achievements of contemporary sociology. But any serious attempt by a great logician and philosopher to come to grips with these problems should be welcomed by all social scientists, who can only gain by a dispassionate appraisal of it.

PERCY S. COHEN

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[1] E. Durkheim, *The Rules of Sociological Method* (trans. S. A. Solovay and John H. Mueller, ed. George E. G. Catlin), Illinois, 1950, pp. 118–120

REVIEWS

The Problem of the Unity of the Sciences: Bacon to Kant. By Robert McRae. University of Toronto Press; Oxford University Press, London, 1961. Pp. xii + 148. 36s.

The ' Unity of Science ' movement has received considerable support during the present century, particularly through the alliance into which it entered with the philosophical school of logical empiricism. At the same time, as Neurath emphasised in his *Encyclopedia of Unified Science*, it was conceived as continuing the work of the famous French Encyclopedists of the eighteenth century. And where logical empiricism aimed at unity through its doctrines of logic and language, the ' encyclopedic ' criterion of unity lay at the other extreme, being of a social nature, as the aggregate of the existing pure and applied sciences, or better even: of scientific topics.

Professor McRae's book reminds us that this is not new. Not only does the ' unity of science ' conception reach back to the beginnings of the seventeenth century if not earlier, thus being contemporaneous with the rise of modern science itself; it furthermore stimulated the spirit of enthusiasm with which that science was pursued, providing a universal point of view. But even in detail, the modern criteria have their ancestors. One of the broad ways in which the unity of science movement arose during the centuries was through the idea of the unity of system. At one extreme we get the idea of unity through logical integration, as found in the doctrines of Descartes and Leibniz, through the guise of the model of mathematics or of logic. At the other end we get the view of writers like Diderot and d'Alembert, opposed to any sort of an artificial system of logical filiation, putting their trust in a set of cross-references to scientific topics, which was as they themselves pointed out ' the most important part of our encyclopaedic scheme '.

A second broad method of the conception of unity lay in that of the ' unity of method '. Here again, logic and mathematics suggested an early optimism that there might be one and only one way of advancing science; Descartes tried to reach it through an analysis of the nature of human knowledge itself.

Curiously enough, Kant comes out against any of these ways of conceiving the scientific progress as a unitary and unifying stream of reducible subject matter, be it in the form of mathematics or of a Leibnizian ' alphabet of human thoughts '. Broadly, where the others employ the guidance of mathematics and logic, Kant's paradigm is that of an organic whole, each of whose parts are relatively independent, and organised under the guidance of a ' regulative idea ', though this is not worked out in any detail. One might expect him therefore also to be inimical towards the claims of a universal ' logic of scientific discovery ', and this is indeed the case. ' Logic is not a general art of discovery, nor an organon of truth; it is not an algebra by means of which hidden truths may be discovered', in the author's quotation from Kant's *Logic*.

This is a scholarly work, written with sympathy for its period, which includes a detailed account of the contributions to the author's survey of Bacon, Descartes, Leibniz, Condillac, Diderot, d'Alembert, and Kant; the question being, to ascertain how the unity of science idea presented itself to these philosophers as a specifically philosophical or logical problem. It is therefore not—as the author emphasises—a 'history of ideas', but offers philosophical appraisal and criticism. It makes a solid contribution towards relating one of the contemporary movements of thought to critical ancestors, thereby enriching our understanding of both.

G. BUCHDAHL

Realism and the Background of Phenomenology. Edited by R. M. Chisholm. Free Press, Glencoe and Allen and Unwin, London, 1960. Pp. viii + 304. 42s.

THIS is a volume in the well-known *Library of Philosophical Movements*. It consists of a series of extracts from the writings of philosophers associated with phenomenology or with realism and is prefaced by an essay by Professor Chisholm. There is, as usual in Free Press publications, a very full and useful bibliography. The writings selected are a dozen pieces from Brentano, Meinong, Husserl and twentieth-century exponents of realism such as Prichard, Russell, and Moore.

The translations from the German are useful chiefly because the originals have been difficult to come by and have not previously been translated. The second group, though much superior in philosophical importance, includes some pieces (for example, Moore's *Defence of Common Sense*) too well known to bear reprinting. The most valuable part of the book is Chisholm's 35 pages of introduction. I could have wished for more of this at the expense of some of the less intelligible of the philosophers here represented.

In spite of the editor's introduction, it is not easy to see the connection between Brentano, Meinong, and Husserl on one hand, and the twentieth-century realists on the other. The passages from the continental writers seem merely to provide further evidence of the genius of German philosophers for constructing pseudo-sciences (like phenomenology) out of truisms expressed in pretentious language. And who can really take Meinong seriously when he heads a section of his essay on the theory of objects with the phrase 'The Prejudice in Favour of the Actual'. The whole essay reads like a caricature of the most fantastic kind of metaphysics. The realists seem a little dated but only in the sense that their work is now a part of the history of philosophy. The editor's introduction is useful on Brentano, some of whose ideas he has developed in his own book on perception. And he has a very clear summary of the implications for the problem of perceiving of the so-called 'subjectivist argument'.

D. J. O'Connor

ERRATUM

An error has crept into my article 'The Status of Determinism',[1] making nonsense of a crucial sentence. On p. 117, tenth line from the bottom, the word not at the end of the line should be deleted.

Jonathan Bennett

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The British Journal for the Philosophy of Science

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RATIONAL DEDUCTION IN PHYSICS: THE PARALLELOGRAM OF FORCES*

L. R. SAUNDERS

1 *Introduction*

THIS paper presents a brief and elementary discussion of the question whether a type of physical proposition is rationally derivable or merely empirical. Once a proposition is endowed with a rational derivation it achieves a sanctity not given to the mass of physical propositions. Consequently it can be argued that the rational derivations made by Eddington and certain others deserve to be taken more seriously, even though the results cannot be called necessarily true.

The major part of the paper presents a deduction of the parallelogram-law for forces, from a small set of very fundamental principles. This deduction illustrates the essential features of the kind of rational derivation under discussion, and is the basis of a concluding section in which it is suggested that certain fundamental postulates would be held in any world, while others would not be introduced unless the nature of the world favoured them. However the postulates are not empirical statements, since the postulate-makers are not *constrained* to consider the nature of the world.

2 *Rationally Derivable Facts*

A physical fact can be rationally derived, contingent or a postulate, and the category to which a particular fact belongs is not always clear. For example, until Archimedes calculated the ratio of the circumference to the diameter of a circle, the determination of that ratio was a matter for experiment, and its value appeared to be a contingent fact. Archimedes showed it to be rationally derivable. A significant feature of rationally derivable facts is that rational derivation is always

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* Received 25.iv.63

allowed to have precedence over empirical determination. Thus, if a figure traced out by a string of radius one foot turned out to have a circumference not equal to 2π feet, we would say that the length of the string had not remained constant.

It is apparent that it is quite possible for a particular piece of information to be rationally derivable from accepted propositions, while apparently contingent. This seems to be the thought underlying the work of Eddington and of Milne, in attempting to extend the range of the *a priori* in physics. Of course physics *as a whole* must contain empirical material, but it could well be that it contains a more extensive rational network than is generally believed. After seeing the spectacular agreement between Eddington's rationally derived fundamental constants with the empirical values, one is inclined to doubt that his work has received its due.[1] A lot has been written on the lack of rigour in much of Eddington's work, and on the impossibility of an *a priori* physics,[2] but it would be pleasant to see a really rigorous study: one which did not merely repeat 'open to doubt' and 'not proven'.[3]

We have referred above to rationally derivable constants, such as π. In addition, there are rationally derivable principles, and later in this paper a demonstration is given that the parallelogram-of-forces law of statics belongs to that class.[4] It is possible to regard that law as contingent, as would be concluded on observing that all heavy chains take the form of a catenary, provided they are uniform (the property of uniformity can be checked beforehand by conformity to an external criterion). However the law is rationally derivable, and is therefore logically linked to the external criterion for uniformity. On the other hand, the law could be made a postulate of statics, in which case it would itself be the criterion for uniformity. But a postulate which turns out to be implied by simpler postulates ceases to be a postulate.

In the present day the physicist Alfred Landé is engaged in an attempt to found quantum mechanics on a basis in which the rational/empirical ratio is as large as possible.[5] He has shown that the assumption of continuity and symmetry in some functions and relations in

[1] E. T. Whittaker, *From Euclid to Eddington*, Cambridge, 1949

[2] J. W. Yolton, *The Philosophy of Science of A. S. Eddington*, Martinus Nijhoff, 1960; G. J. Whitrow, *The Structure and Evolution of the Universe*, Hutchinson, 1959, Chap. 6

[3] Groundwork for this task has been laid by N. B. Slater in his *The Development and Meaning of Eddington's 'Fundamental Theory'*, Cambridge, 1957.

[4] Throughout this paper statics is considered in isolation from dynamics. Thus Newton's law $F = ma$ is not invoked.

[5] Alfred Landé, *Foundations of Quantum Theory*, Yale, 1955

thermodynamics leads to a set of conditions on the probability amplitude function. The only function known to satisfy these conditions is the long-established Schrödinger wave function. Although it is unlikely that any simpler solution exists, uniqueness has not been proved. Thus, the wave solution has not been shown to be rationally derivable, though it may well be so.

Landé's problem is similar in some respects to that of the parallelogram of forces, and was in fact the reason for the present writer's work on that (far simpler) problem. Before commencing it, we shall consider Stevinus's historic presentation of the law.

3 *The Argument of Stevinus*

The parallelogram of forces was discovered in the first place by the Dutch engineer Stevinus (1548–1620).[1] Stevinus considered an endless chain supported by a smooth triangle whose base was horizontal.

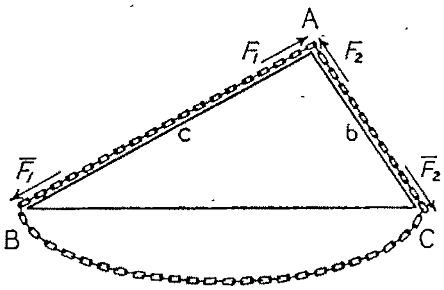

FIG. 1

First of all, the chain must be uniform, and presumably this would be said to be so if equal lengths balanced when suspended over a pulley (a pulley is equivalent to a triangle with a base of zero length). By making one length a standard, the inherent weight-force of a given section of chain becomes proportional to its length. Next, it has to be assumed that this vertical weight force is independent of the horizontal location of the section or its orientation (in *a posteriori* terms, the gravity-field is uniform).

Now either a position of equilibrium exists or else the chain has an acceleration which never changes sign. On assuming the impossibility of perpetual motion, a position of equilibrium must exist.

---

[1] J. H. Jeans, *The Growth of Physical Science*, Cambridge, 1947, Chap. 5

Stevinus next *assumed* that the equilibrium of the upper portion of the chain would not be disturbed if the hanging portion were removed. As an alternative, it could be assumed that the hanging portion is symmetrical, so that the tension forces $\bar{F}_1$ and $\bar{F}_2$ are equal. Also, the forces F_1 and F_2 are equal. Consider now the equilibrium of the portion along AB, and let the effective force along AB due to the weight be proportional to the function f(B) of the angle of slope. At once we have

$$F_1 = \bar{F}_1 + \lambda c f(\mathrm{B}),$$

where λ is the linear density.

Similarly,

$$F_2 = \bar{F}_2 + \lambda b f(\mathrm{C}).$$

(c and b are the lengths of AB and AC).

Consequently,

$$\frac{f(\mathrm{B})}{f(\mathrm{C})} = \frac{b}{c} = \frac{\sin \mathrm{B}}{\sin \mathrm{C}}.$$

Hence $f(\mathrm{B})$ is proportional to *sin* B, and on considering the case $\mathrm{B} = 90°, f(\mathrm{B}) = \sin \mathrm{B}$. This result leads immediately to the standard parallelogram law, the triangle of forces, and Làmi's theorem.

The conclusion was established without recourse to actual experiment, but the method was not *entirely* rational. It was prevented from being so by the assumption concerning the possibility of removing the hanging portion of the chain. We know that before one can *prove* that the hanging part must be symmetrical (in a uniform gravity field), the parallelogram law has to be assumed. Nevertheless there is sufficient of the rational in this ingenious argument to account for Stevinus's exclamation 'Here is a wonder and yet no wonder'.[1]

4 *A Rational Deduction of the Parallelogram law*

The parallelogram law can be derived in a more basic way by identifying the magnitude of a force with the extension of a spring balance. This shall be the operational meaning of force magnitudes. In this way the concept of gravity and the consideration of the equilibrium of a heavy chain can be avoided.

Suppose we have a number of spring balances, and let us take one of them as a standard. Each of the others is calibrated by coupling its hook with that of the standard, and comparing extensions under equilibrium. It follows at once that two forces acting in opposite senses along a line are in equilibrium if and only if their magnitudes are equal. Let us call this 'Principle 1' (P_1). Now consider three spring balances coupled together in accordance with the force diagram below.

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[1] W. T. Sedgwick and H.W. Tyler, *A Short History of Science*, Macmillan, 1927, p. 252. The account of Stevinus's argument is not satisfactory.

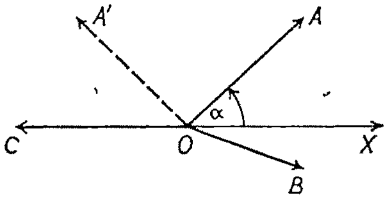

FIG. 2

The forces are in equilibrium, and in particular there is no tendency for O to move in the X direction (COX is a straight line). Let us keep the force C constant in magnitude and direction, and consider the *influence* of A and of B in the X direction. But first we must introduce a postulate P_2: In a system of forces, each force acts independently of the others.

We define the influence of A in the X direction as $\psi(\alpha)$, where α is the angle XOA, and introduce a principle of symmetry.

P_3: ψ is an even function of α.

This is equivalent to stating that on making A act at $-\alpha$, the influence is unaltered. Clearly $\psi(\alpha)$ must be periodic, of period 2π. Our fourth principle states that $\psi(\alpha)$ satisfies certain continuity conditions.

P_4: $\psi(\alpha)$ is bounded and continuous, its first derivative is continuous, and its second derivative is at least sectionally continuous.

P_3 and P_4 imply that $\psi(\alpha)$ is expressible as a Fourier cosine series (actually this would be so even if P_4 were less stringent). Suppose now that A is caused to act as A' at the angle $\pi - \alpha$. By P_1 the influence is the negative of the influence of A acting at $-\alpha$; and by P_3 it must therefore be the negative of A acting at $+\alpha$. Consequently the Fourier series has terms only of odd multiples of α.

Thus, $\psi(\alpha) = a_1 \cos \alpha + a_3 \cos 3\alpha + a_5 \cos 5\alpha + \ldots$ (1)

By P_1, $\psi(0) = 1$; whence $a_1 + a_3 + a_5 + \ldots = 1$. (2)

We now introduce a second symmetry postulate.

P_5: A set of n radial forces of equal magnitude and inclined at the angle $2\pi/n$ to each other is in equilibrium, for $n = 3,5,7, \ldots$ (that this is so for $n = 2,4,6, \ldots$ follows at once from P_1). That P_5 is a symmetry principle can be seen from the following consideration. If equilibrium did not obtain, O would tend to move in a direction inclined at an angle δ to one of the forces. But by symmetry $n-1$ other directions have an equal *a priori* claim, so that no direction can be supported.

Now let ϵ be a positive number. Then there is a positive number M such that for $m > M$, $|\psi_m(\alpha) - \psi(\alpha)| < \epsilon$, for all α, (3)
where $\psi_m(\alpha)$ is the $(2m+1)$th. partial sum of the Fourier series, defined by

$$\psi_m(\alpha) = a_1 \cos \alpha + a_3 \cos 3\alpha + \ldots + a_{2m+1} \cos(2m+1)\alpha \tag{4}$$

Suppose we have the symmetrical set R_{2m+1} of $2m+1$ equal forces. P_5 implies equilibrium, so that

$$|\psi_m(\theta) + \psi_m(2\theta) + \ldots + \psi_m\{(2m+1)\theta\}| < (2m+1)\epsilon \tag{5},$$

where $\theta = 2\pi/(2m+1)$.

i.e. $|a_1\{\cos\theta + \cos 2\theta + \ldots + \cos(2m+1)\theta\}$
$+ a_3\{\cos 3\theta + \cos 6\theta + \ldots + \cos 3(2m+1)\theta\}$
$+ \ldots$
$+ a_{2m+1}\{\cos(2m+1)\theta + \cos(2m+1)2\theta + \ldots \cos(2m+1)^2\theta\}|$
$< (2m+1)\epsilon$ (6)

It can be shown that $\sum_{r=1}^{2m+1} \cos kr\theta = 0$, unless $k\theta$ is a multiple of 2π, in which case the sum has the value $2m+1$. Hence the only non-vanishing term of (6) is the last, and we have

$$|a_{2m+1}| < \epsilon.$$

In exactly the same way, by considering the set R_{2m-1}, we can show that $|a_{2m=1}| < \epsilon$, unless $2m+1$ is a multiple of $2m-1$, in which case we have $|a_{2m=1} + a_{2m+1}| < \epsilon$. To see this clearly, suppose $m = 7$. By considering the equilibrium of R_{15}, R_{13}, R_{11}, $\ldots$, R_3 we obtain $|a_{15}| < \epsilon$, $|a_{13}| < \epsilon$, $|a_{11}| < \epsilon$, $|a_9| < \epsilon$, $|a_7| < \epsilon$, $|a_5 + a_{15}| < \epsilon$, $|a_3 + a_9 + a_{15}| < \epsilon$. The last two inequalities yield $|a_5| < 2\epsilon$, $|a_3| < 3\epsilon$.

In general, $|a_k| < \gamma\epsilon$, where $\gamma = O(2m+1)$. Now from the theory of Fourier series, it can be deduced that for a function $\psi(\alpha)$ satisfying the particular continuity conditions of P_4, m and ϵ are related by $\epsilon = O\{(2m+\frac{3}{2})^{-\frac{3}{2}}\}$.[1] Hence $a_k = O(2m+1)$. Since m can be taken as large as we please, the coefficients a_3, a_5, $\ldots$ are arbitrarily small.

Consequently $\psi(\alpha) = a_1 \cos\alpha$, (7)
and by (2), $a_1 = 1$.

This completes the proof.

5 *General Discussion*

It has been shown in the previous section that the parallelogram law follows from

---

[1] See C. Lanczos, *Linear Differential Operators*, Van Nostrand, 1961, equation 2.4.16. Since $\psi(\alpha)$ satisfies P_4, we put $m = 2$ in Lanczos's equation.

(*a*) the definition of the magnitude of a force (P_1)
(*b*) an independence assumption (P_2)
(*c*) a continuity assumption (P_3)
(*d*) symmetry assumptions (P_4 and P_5).[1]

It was tacitly assumed that nothing was operating but the forces in question. The symmetry assumptions amount to stating that space is isotropic, and that an observer on a swivel chair at O will note the same effects regardless of the angular position of this chair, or whether he changes from observing from above to observing from below. Hence, taking the observer as irrelevant, any effects contrary to symmetry would cause us to say that the original model did not cater for the circumstances (so that further concepts would need to be introduced).

To be specific, suppose that three spring balances in a symmetrical array gave equilibrium while indicating unequal forces. This anomaly could be coped with in many ways, the particular way to be adopted being dependent on the empirical data. For example, one could say that a spring constant is a function of the angular orientation of a spring balance in space. (At first sight this might appear to clash with P_1, but this is not so.) However if the experimental result was independent of spatial orientation an 'interaction' explanation would be adopted. In fact the departure from the parallelogram law could be used as a measure of interaction.

If the interaction effect were always present in exactly the same way, it is likely though not necessary that we would dispense with interaction as a concept, and have a different kind of statics from that which we now have. In any case, neither the symmetry nor interaction assumptions are rigorously determined by the nature of the world. Even if all experience went counter to them, they could still be retained.

The continuity assumption is a little more difficult to consider. It is true that in mechanics and electromagnetism all functions are analytic except at particles or boundaries, so that it is possible to regard such a discontinuity as the *meaning* of a boundary. However suppose we make experiments with three spring balances calibrated as described previously. It is conceivable that discontinuous behaviour could be observed while continually altering the configuration of Fig. 2. If this discontinuous behaviour were universal, and independent of orientation in space, to 'explain' it by the idea of a physical boundary would necessitate a radical enlargement of that concept. Almost certainly, we would instead accept force as an inherently discontinuous phenomenon.

---

[1] Assumptions of continuity and symmetry are the chief premisses in Landé's development of quantum mechanics (op. cit.).

However we would not be *constrained* to reject continuity—we could retain continuity as a principle, and state as an *empirical* contention that collateral with a collection of forces is a phenomenon which appears as a discontinuity. This procedure would be analogous to that which is implicit in the theory of gases, to mention just one of many cases. There is a concept of a perfect gas to which further concepts must be added in order to characterise the behaviour of an actual gas. Doubtless, the concept of a perfect gas would not be held if there were no real gases which under certain conditions behaved very like a perfect gas. But this does not imply that a statement asserting or denying the existence of a perfect gas is an empirical one, though it is true that matters of fact have a bearing on whether the concept is held or not. (Suppose a corrupt arbitrator faced with choices *A* and *B* makes the decision *B* as a consequence of a bribe. We do not say that the bribe was evidence supporting *B*, though it certainly had a bearing.)

To return to the continuity assumption, it can be seen that in order to hold to it in the face of hostile empirical data, we would need a new concept to cater for the phenomenon of discontinuity. If the phenomenon were of universal occurrence, it would clearly be more economical to abandon the continuity assumption so that the new concept would not be needed.

Evidently the same considerations apply to all the postulates. The nature of the world has a bearing on the existence of such a postulate through the fact that physics-makers apply a rule akin to Occam's razor. That is, it is considered undesirable for a theoretical system to have surplus concepts with regard to its application to phenomena. Thus, the allowing of a particular postulate is dependent on human fiat, and since human fiat does not *have* to bow to the nature of the world, the postulates are distinct from empirical statements. (Actually the latter are expressed in terms of the vocabulary supplied by the former.)

On the other hand the postulates are not analytic, since they can be denied without self-contradiction. Thus there is some attraction in applying the term 'synthetic *a priori*' to such postulates, though of course the term cannot be taken exactly as Kant used it, since we cannot claim 'necessity'. Alternatively, it could be argued that the postulates are components of the *definition* of an ideal force system, so that any *deduction* from them is analytic. However the question of the respective merits of these two alternatives would be a lengthy one to deal with, and outside the scope of this paper.

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DEDUCTIVE UNIFICATION AND IDEALISATION *

S. KÖRNER

APART from a sprinkling of Hegelians, modern philosophers of science are on the whole either moderate Humeans or moderate Kantians. The moderation of the Humeans shows itself in their rejection of Hume's doctrine that all belief can be reduced to impressions and ideas ; that of the Kantians in the rejection of transcendental arguments to the effect that a categorial scheme, which is in fact accepted by somebody, *must*—in an interesting sense of ' must '—be accepted by everybody. The Humeans and Kantians of today agree with each other and their orthodox predecessors that theoretical reason has no constitutive function or, in more modern terminology, that the deductive unification of a field of empirical enquiry is merely an ordering, and not a modification, of the concepts and propositions describing this field.

I shall try to show that on the contrary the deductive unification of a field of experience *ipso facto* involves the replacement of empirical predicates and propositions by non-empirical ones, and is thus not a mere ordering but an idealisation. The talk is divided into three parts. First, the classical account of the function of hypothetico-deductive systems will be criticised. Next, some logical questions, which arise from the criticism, will be considered. In conclusion, I shall outline an alternative account of the function of hypothetico-deductive theories and some of its implications for the understanding of the relations between science and extra-scientific thinking.

I

The classical account of scientific prediction and explanation by means of a hypothetico-deductive theory T, e.g. classical mechanics, is very briefly this : Starting with a basic proposition, say b_1, which describes what has been found by experiment or observation, one deduces from b_1 and T another basic proposition b_2, which can again be compared with experiment or observation. Schematically,

$$(b_1 \text{ and } T) \vdash_{\text{L}} b_2.$$

---

* This paper was read to the British Society for the Philosophy of Science 20.v.63.

In this formula '$\vdash_{L}$' expresses deducibility with respect to an underlying logic *L*, comprising as a rule the two-valued logic of propositions, quantification theory, and the theory of equality, and often extended so as to accommodate the theory of real numbers.

The predicates of *T* are empirical or non-empirical (' theoretical '). The constituent predicates of the basic propositions are included in the empirical predicates of *T*. There exist different variants of the classical account, according to the way in which the basic propositions are distinguished from other empirical propositions, and the way in which empirical and non-empirical predicates and propositions are distinguished from each other. There is no need to discuss these differences here.

From the point of view of a working scientist and a philosopher of science who is interested only in the structure of scientific theories, and not also in their relations to extra-scientific thinking, the classical account has many merits. However, from the point of view of a philosophy which aims at an understanding of these relations—one would almost say, from the point of view of a philosophical philosophy of science—it is open to objections. These are all connected with restrictions imposed by the underlying logic of scientific theories upon their predicates and propositions. Recognition of the restrictions counteracts illegitimate tendencies to extrapolate the theories beyond the range of their applicability.

A hypothetico-deductive theory can, as is well known, be regarded as an extension of its underlying logic. This extension consists in adding to the logical terms and predicates the substantive terms and predicates of the field of empirical enquiry which is unified by the theory ; and in adding to the logical postulates the substantive postulates which govern the use of the substantive terms and predicates. The possible logical relations between propositions are not affected by the extension. Thus adjunction, negation, class-inclusion, etc., are determined by the underlying logic.

The first, and most obvious, restriction imposed by *L* on *T* concerns theories the underlying logic of which is sufficiently rich to contain the classical mathematics of real numbers. If the basic propositions are about any kind of continuous expanse or transition, they will be formulated in terms of a mathematical notion of continuity, conforming to Dedekind's or some similar mathematical postulates. These include, among others, the so-called postulate of density according to which between any two members of a continuous series there exists at least one further member and therefore an infinite number of members. The class of these members has to be thought of as a given totality which as follows from the postulates of the theory, is non-denumerable.

It is clear that the mathematical notion of continuity is an idealisation of an empirical notion. I have argued elsewhere[1] that the notion of empirical continuity is neither, as is sometimes asserted, unanalysable nor self-contradictory, but that it can be analysed in terms of a finite number of inexact classes. The classical logic which recognises only exact classes, which can in no way be regarded as merging into each other, counterbalances, as it were, this idealisation by the further idealising assumption of non-denumerable infinite totalities. Without going into a more detailed analysis of either empirically or mathematically continuous series, the latter are certainly not descriptions, but at best, idealisations of what is observed.

The second, and hardly less obvious, restriction imposed by L on T concerns an even wider class of theories, namely all those, the underlying logic of which contains the theory of equality. The difference between the logico-mathematical and the empirical notion of equality has often been pointed out. To avoid misunderstandings, it might be better to speak of perceptual or operational indistinguishability instead of equality. Poincaré and many others after him have shown that, whereas mathematical equality is transitive, perceptual equality is not. To give an often quoted example : If we consider empirical equality or indistinguishability with respect to weight—weight being determined by a pair of scales—then, since no pair of scales is infinitely sensitive, it is always possible to find three objects A, B, C, such that A balances B, B balances C, but A does not balance C.

Similar considerations apply to empirical equality in length and in temporal duration. All these notions are non-transitive and the empirical propositions involving them are on incorporation into a theory, the underlying logic of which contains the theory of equality, idealised into propositions involving transitive relations of equality. The idealisation affects not only the fundamental quantities of physics but clearly also the derived quantities, which are defined in terms of them. Indeed, if in a very wide sense of the term we call ' quantitative ' all theories embedded in a logic with a transitive equality-relation, then all quantitative theories contain basic propositions which are, strictly speaking, idealisations of experience.

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[1] *Monist*, 1962, 47, 1-19

I now turn to an idealisation which is imposed on all theories, even those that are not quantitative, by the structure of their underlying logic. About this logic I assume only that it comprises the two-valued logic of propositions and the predicate logic of first-order or quantification-theory. In order to clear the ground I shall first of all very briefly consider the manner in which experience of the world around us is differentiated into individuals and classes. Many schemata of empirical differentiation, i.e. of individuation and classification, are conceivable and there is no reason to suppose that one of them is logically prior to any other. I should be prepared to argue that arguments to the contrary (so far given and known to me) rest either on a *petitio principii* or on a *circulus vitiosus* the radius of which is usually too small to make it either interesting or excusable.

The most familiar, and perhaps the oldest, schema of empirical differentiation divides the world around us into individual things moving, or capable of being moved, in empty space and time. Others are the schema of things in fields and the schema of thingless fields in which changing patterns are discerned. One cannot predict which as yet unconceived schemata of empirical differentiation will arise from the ever-changing language of ordinary discourse, from science, metaphysical speculation and their interaction.

As far as these schemata are concerned—and I can see no reason why what holds of the known ones should not also hold for those of the future—the definition of empirical predicates and the classes corresponding to them, depends on the discernment of similarities and dissimilarities. Their definition in terms of other empirical predicates requires in the end the exhibition of examples of a predicate's applicability and inapplicability, i.e. of standard members and non-members of the corresponding classes. This is to say that among the qualifications and disqualifications of an individual for falling under a predicate, or for its membership of the corresponding class, there is the requirement that it should be similar to standard members and dissimilar from standard non-members—there being a method available for producing standard objects. I shall call any predicate or class which is characterised in the manner mentioned a ' resemblance-predicate ' or ' resemblance-class '.

The way in which resemblance-predicates and classes are defined, or introduced into language, accounts for their inexactness, i.e. their admitting of borderline cases. Apart from having standard members and non-members, and ordinary members and non-members (i.e.

objects which are sufficiently similar to standard members and dissimilar from standard non-members to qualify for membership ; and objects which on the basis of the comparison with standard members and non-members are clearly disqualified for membership), a resemblance-class also admits of neutral candidates for membership of the class, i.e. of objects which can with equal correctness be assigned or refused membership of it.

Just as some empirical predicates and classes are inexact, so some empirical individuals are indefinite in the sense that whereas some individuals are clearly their parts and others clearly not, there are yet other individuals which with equal correctness can be judged to be or not to be their parts. This may be so, whether the parts in question are spatial or temporal. If examples are needed, one might instance the gradual separation of one or more parts from a complex individual, or the difficulties of determining the beginning or end of an individual's, especially an animal's, history.

It may be worth emphasising that an individual's indefiniteness in some aspect or phase is compatible with its definiteness elsewhere. Some individuals are as clearly parts or non-parts of others, as they are members or non-members of resemblance-classes. Although for some purposes it is useful to treat the indefiniteness of individuals independently of the inexactness of predicates, it should be noted that the former notion can be explained in terms of the latter. The empirical predicate 'x is a spatio-temporal part of y' is clearly a resemblance-predicate ; and every neutral instance of this predicate is an ordered couple, the first member of which can with equal correctness be judged to be or not to be a part of the second. I shall, therefore, say no more of the indefiniteness of empirical individuals.

After these remarks it is not difficult to see that every hypothetico-deductive theory, which unifies a field of experience described in terms of resemblance-predicates, and is embedded in the classical two-valued logic, enforces the replacement of these predicates by idealisations of them. Whereas in the classical logic, if O is an individual and P is a predicate, only two kinds of predication are possible namely, (i) $o \epsilon P$, i.e. 'o is an instance of P' and (ii) $o \epsilon' P$, i.e. 'o is not an instance of P', resemblance-predicates admit also of a third possibility, namely (say) (iii) $o \epsilon^{\star} P$, i.e. 'o is a neutral candidate for P'. To these three analysed propositions, there correspond three unanalysed atomic propositions, namely (i) p, i.e. 'p is true', (ii) $7p$, i.e. 'p is false' and (iii) $\star p$, i.e. 'p

is neutral'. It is clear that classical two-valued logic cannot accommodate the third possibility.

2

In looking for a logical system which will do justice to the possibility that in addition to atomic propositions of form $(o\epsilon P)$ or p and negated atomic propositions $(o\epsilon' P)$ or $7p$, there are to be considered also neutral propositions $(o\epsilon^{\star} P)$ or $^{\star}p$, we must be guided by the following considerations. Firstly we must observe the principle of permanence. Since not all propositions are neutral and not all predicates inexact, the rules governing their composition and interrelations must be so chosen that for propositions that are not neutral and classes that are exact, they reduce to the rules of classical logic (or, if we prefer, of intuitionist logic, which I shall not consider here). Secondly, it must be observed that neutrality is not an independent third truth-value. It may be turned into either of the other two, i.e. truth or falsehood. Neutrality is neutrality between truth-values. This rules out the Łukasiewicz-Tarski type of three-valued logic (and also, as I am inclined to think, modal logic).

It is fortunate that a logic which is suitable for our purpose is—on the whole, apart from slight modifications—already available. It has been constructed by Kleene[1] in order to do justice to reasoning involving partial recursive predicates (i.e. predicates which within their range of definition are algorithmically decidable, but for which so far no algorithm is available, by which to decide, for any given object, whether it does or does not fall within the predicate's range of definition). Kleene distinguishes the following possibilities : (i) an algorithm is available for deciding whether a given object o falls under a given predicate or not, i.e. (*a*) $(o\epsilon P)$ (*b*) $(o\epsilon' P)$ and (ii) the possibility that so far no such algorithm is available, a situation which may be indicated by $(o?P)$. However, this situation need not be final since an algorithm might be found which would turn $(o?P)$ into either $(o\epsilon P)$ or $(o\epsilon' P)$.

Now the important similarity between $(o?P)$ and $(o\epsilon^{\star} P)$ lies in the fact that, as Kleene puts it, the third truth-value is in both these cases 'not on a par with the other two'. Both $(o?P)$ and $(o\epsilon^{\star} P)$ are provisional, but whereas in the case of $(o?P)$ we may never find an algorithm which will turn it into $(o\epsilon P)$ or $(o\epsilon' P)$, in the case of a neutral candidate o, we may always decide to turn $(o\epsilon^{*}P)$ into $(o\epsilon P)$ or $(o\epsilon' P)$ and the corresponding neutral proposition $^{*}p$ into p or $7p$. In the first, the Kleene-case, the provisional evaluation may, but need not, be followed by a final evaluation. In the other case, the case of neutrality, the provisional can always be followed by a final evaluation, since it is a matter of decision only.

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[1] See especially §64 of his *Introduction to Metamathematics*, Amsterdam, 1952

I shall now, following Kleene, outline the first order predicate logic *prior to the final evaluation* of neutral propositions. The relevant truth-tables are :

$7p$

p	$7p$
t	f
f	t
*	*

$p \wedge q$

	q	t	f	*
p	t	t	f	*
	f	f	f	f
	*	*	f	*

$p \vee q$

	q	t	f	*
p	t	t	t	t
	f	t	f	*
	*	t	*	*

$p \rightarrow q$

	q	t	f	*
p	t	t	f	*
	f	t	t	t
	*	t	*	*

Quantification theory can be developed on the usual lines by treating universal quantification and existential quantification on the analogy of conjunction and adjunction. Thus the proposition $(x)P(x)$ is true if, and only if, all substitution-instances of P are true, false if at least one is false, and neutral if at least one of them is neutral and the others neutral or true. Similarly $(\exists x)P(x)$ is false, if and only if, all the substitution-instances of P are false, true if at least one of them is true, and neutral if at least one of them is neutral and the others false. Classes are also introduced in the usual way by stipulating that two predicates $P(x)$ and $Q(x)$ determine the same class : $(\lambda x)P(x) = (\lambda y)Q(y)$ if, and only if, for any object o the substitution-instances $P(o)$ and $Q(o)$ are either both true, both false or both neutral.

Since neutral propositions can always be turned by our decision into true or false ones, we can always distinguish between a provisional and a final stage of evaluation.[1] In the provisional stage we ignore all neutral components together with their connectives, whereas in the final stage we evaluate them as true or false. Writing square brackets for the provisional and braces for the final stage we can distinguish between the following cases :

I. (*a*) [ ], $\{t, f\}$

In the provisional stage the proposition is neither true nor false ; in the final stage we have truth or falsehood.

(*b*) [ ] $\{t\}$

(*c*) [ ] $\{f\}$

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[1] For an earlier formulation of this distinction see ch. VIII of my *Philosophy of Mathematics*, London, 1960, where it is applied to the logic of classes.

In the provisional stage we have neither truth nor falsehood, but in the final stage we have either only truth or only falsehood. For example, if p_0 is neutral and q_0 is false, then p_0 illustrates (*a*), $(p_0 \vee \neg p_0)$ illustrates (*b*) and $(p_0 \wedge \neg p_0)$ illustrates (*c*).

II. (*a*) $[t]$, $\{t\}$
(*b*) $[f]$, $\{f\}$

The value at the provisional stage is always preserved in the final stage. For example $(\neg q_0 \vee p_0)$ illustrates (*a*) and $(q_0 \wedge p_0)$ illustrates (*b*).

III. (*a*) $[t]$, $\{t, f\}$
(*b*) $[f]$, $\{t, f\}$

The value of the provisional stage is not always preserved but may change in the final stage. For example, $(\neg q_0 \wedge p_0)$ illustrates (*a*) and $(q_0 \vee p_0)$ illustrates (*b*). There is little point in giving special names to these possibilities, which hold also if the propositions under consideration contain quantifiers.

Since the provisional stage of evaluation is always succeeded by a final stage in which neutrality has been eliminated, I shall call the system here developed in outline, the modified two-valued logic. As regards the meaning of a logical identity in this logic we may choose between a weaker and a stronger definition. According to the stronger definition a proposition of the modified two-valued logic is an identity if, and only if, both its provisional and its final evaluation yield a classical identity ; according to the weaker definition a proposition of the modified two-valued logic is an identity if, and only if, the final evaluation yields a classical identity. If we accept the weaker definition then the identities of classical logic coincide with the identities of the modified logic.

It would, however, be a mistake to conclude that there are no important differences between the two systems. From the point of view of this talk, the most important difference is that the modified two-valued logic cannot in general serve as an instrument for the deduction from neutral statements, and statements with inexact predicates as constituents, of other such statements—in conformity with the usual rules of inference, especially *modus ponens*, which though dispensable in predicate logic is not dispensable in an axiomatic theory embedded in it.

Although the form : $((p \wedge (p \rightarrow q)) \rightarrow q)$ is an identity, we are allowed to deduce a proposition q_0 from the premisses p_0 and $(p_0 \rightarrow q_0)$ only if these premisses are either true or, in case they are neutral, evaluated as true. This means that premisses which in their final evaluation can be true or false are treated *as if* they could be evaluated as true only.

In other words we treat statements characterised by [ ], $\{t, f\}$; or $[t]$ $\{t, f\}$; or $[f]$, $\{t, f\}$ as if the second member of each pair were $\{t\}$ and *not* $\{f, t\}$. In applying the *modus ponens* we thus ignore the neutrality of propositions.

A more radical modification is required if one of our contingent premisses is a universally quantified proposition involving an inexact predicate e.g. $(x)P_0(x)$, with P_0 inexact. For it is then not sufficient to turn a single neutral substitution-instance of $P_0(x)$, e.g. $P_0(x_0)$, where x_0 is a neutral candidate for P_0, into a true proposition. To avail oneself of $(x)P_0(x)$ as a premiss, *all* neutral substitution-instances must be evaluated as true, whether or not they are specified, unspecified, or indeed specifiable. We treat P_0 *as if* it had no neutral candidates, i.e. as if it were exact. In applying *modus ponens* to universally quantified propositions, we thus not only ignore the neutrality of propositions, but also the inexactness of predicates. The same applies also for other rules of inference, e.g. to

$$\frac{C \rightarrow P(x)}{C \rightarrow (x)P(x)} \quad \text{(provided } x \text{ is not free in } C\text{).}$$

To sum up: In so far as we restrict ourselves to logical identities and logically true propositions, there is hardly any difference between the classical two-valued logic and the modified two-valued logic, which unlike the former can accommodate neutral propositions and inexact predicates, and thus resemblance-predicates and propositions with such predicates as constituents. As an instrument of deduction of contingent conclusions from contingent premisses, the classical logic requires that neutral propositions be replaced by definite propositions and inexact predicates by exact predicates. This replacement, which is enforced by a transition from the modified two-valued to the classical two-valued logic, shows that deductive unification of an empirical field by means of classical logic is not a mere ordering but an idealisation.

3

Since the basic propositions of a hypothetico-deductive theory function as premisses from which together with the postulates of the theory other basic propositions are deducible, the constituent predicates of the basic propositions are not descriptive, but idealisations, of experience. A basic statement of a continuous transition involves the replacement of an empirical notion of continuity, definable in terms of a finite set of inexact classes, by a mathematical notion of continuity, defined in terms of a non-denumerably infinite set of exact classes. A basic statement of an equality involves the replacement of an empirical non-transitive relation by the mathematical relation of equality, which is transitive. Lastly, the incorporation into the theory of any statement with an inexact, in particular a resemblance-predicate, among its constituents, involves its replacement by an exact predicate. Of this replacement the other two are special cases since empirical continuity and empirical equality in a certain respect, are resemblance-predicates.

It is thus, strictly speaking, false to assert the identity of the basic propositions of a theory with the empirical propositions of which they are idealisations. It is, however, still possible and advisable to distinguish between the basic and other propositions of the theory, and to define as basic those propositions which are identified (though not identical) with empirical propositions. The identification is permissible only for some purposes and within some contexts. Which these purposes and contexts are, is in any case an empirical question.

If we write '$b \approx e$' for the relation which holds between b and e when b is an idealisation of e due to the replacement of empirical by idealised predicates in the way explained above, then we can replace the classical schema

$$(b_1 \text{ and } T) \vdash_{L} b_2$$

by the more adequate schema

$$(e_1 \approx b_1)\ ;\ (b_1 \text{ and } T) \vdash_{L} b_2\ ;\ (b_2 \approx e_2)$$

It indicates and emphasises that whereas b_2 is deducible from b_1 and T, none of the e-propositions is deducible from any of the b-propositions or vice versa. Neither is e_2 deducible from the conjunction of e_1, b_1, T and b_2.

Put into words the schema may be read: If within specified contexts and for specified purposes—such as the construction of machines or the prediction of physical events—an empirical proposition e_1, describing a certain type of situation, is (in the manner explained) idealised into a basic theoretical proposition b_1, if b_2 is deducible from b_1 and T, and if b_2 is 'de-idealised' into e_2, then if e_1 is true, e_2 is also true. The following comments on the statement are appropriate. First, it expresses an empirical proposition. Second, one does not, strictly speaking, verify, falsify, confirm or disconfirm the theory, but the whole empirical proposition. In it the theory is only one constituent —as it were, an empirically incomplete symbol. Third, the scientific proposition does not differ from an 'ordinary' empirical proposition such as 'If e_1 is true, e_2 is also true' by being more or less empirical. The difference lies in that the scientific empirical proposition is connected through the theory with other empirical propositions, the idealised counterparts of which are deductively unified by it. Fourth, the identification of empirical with non-empirical predicates and propositions allows for great precision and facility of reasoning and calculation. Last, since the identification is limited to special purposes and contexts, its extension beyond its original limits, though of heuristic value, requires fresh evidence.

I conclude with a brief remark on an example of unwarranted philosophical extrapolation of a scientific theory beyond the limits of legitimate identification, namely the problem of the compatibility or incompatibility of physics and morality.[1] If our bodies are without qualification or limitation subject to the laws of physics, then since we cannot act without our bodies, those of our actions which seem to us determined by free decisions are in fact—with certainty or probability—determined by physical antecedents alone. But since the applicability of theoretical physics to experience, via the identification of empirical with idealised propositions, is limited to contexts in which decisions play no part, it is not theoretical physics, but an unwarranted philosophical extrapolation of it, which is incompatible with the assumption that decisions, for which we are responsible, at least sometimes determine our actions. The compatibility between scientific theories (embedded in classical logic) and moral responsibility does not imply that moral responsibility is, or is not, an empty notion. But it leaves room for the belief that it is not empty—a belief or faith, which is deeply rooted in Western tradition.

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[1] For a fuller statement see 'Science and Moral Responsibility' to be published in *Mind.*, April 1964, 73

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TELEOLOGICAL EXPLANATION IN BIOLOGY*†

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EXPRESSIONS like ' function ', ' purpose ', ' role ' and ' in order to ' occur in sentences in biology. For example : ' A function of liver is to secrete bile ', ' The heart beats in order to circulate the blood ', ' The role of the sodium chloride is to prevent the cells from taking up too much water '. Such sentences have been called ' functional analyses '. Functional analyses containing the expressions ' purpose ', ' role ', ' in order to ' and similar teleological locutions can be rewritten using only ' function '. Thus ' A function of the heartbeat is to circulate the blood ' says what the second example given above says.

Functional analysis raises two closely related philosophical problems. The first centres on the teleological word ' function ' : Does biology contain, irredeemably, sentences utilising teleological notions? Put another way : Can all functional analyses containing the locution ' function ' be translated into sentences which contain neither ' function ' nor any other teleological term?

The second problem centres on functional analyses which serve, or purport to serve, as *explanations*. In some contexts, a functional analysis provides an appropriate answer to a request for an explanation. For example : 'Why does the heart beat?' ' In order to circulate the blood.' Philosophers have thought that when a functional analysis serves as an explanation in such a context, it must be treated as elliptical for some longer sentence. The problem, then, is : When a functional analysis serves as an explanation (i.e. when we have a prima facie teleological explanation) how is the functional analysis to be understood?

Perhaps the standard answer to the first of these problems is that functional analyses can be translated into sentences containing no teleological terms.[1] I believe that this answer is right, but that the reasons

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[1] See R. B. Braithwaite, *Scientific Explanation*, Cambridge University Press, 1953, ch. X ; I. Scheffler, ' Thoughts on Teleology,' this *Journal*, 1958, 9; G. Sommerhoff, *Analytical Biology*, Oxford University Press, 1950 ; and E. Nagel *The Structure of Science*, Harcourt Brace, 1961, ch. 12

that have been given for it are wrong. A standard answer to the second problem is that teleological explanations in biology, when unpacked, fit the covering law model of explanation.[1] This answer, I believe, is wrong.

In this paper I first present and defend a translation schema for taking functional analyses into sentences containing no teleological terms. I then use this analysis of functional analysis in criticising and amending the above mentioned view of teleological explanation.

I

This section gives a preliminary account of functional analysis, and defends the account by examples.

1. A dictionary of biology states : ' THYMUS, organ of vertebrates, usually in . . . neck region, formed embryologically from gill-pouches or gill-clefts. Function unknown. . . .'

What would an account of the function of the thymus look like? What follows in part quotes and in part paraphrases a report of recent experiments on the function of the thymus.[2]

' The standard first step towards elucidating the function of an organ is to remove it and analyze the resulting disabilities.' Until 1961 this approach gave no clues to the function of the thymus. Recent experiments, however, did produce clues. The thymus was removed from day old animals, with significant results. It was found, for example, that ' a variable proportion of mice, rats and rabbits thymectomized at birth lose most or all of their capacity to produce antibodies . . .'. Given these and similar results, a plausible view ' is that the thymus produces and liberates into the bloodstream the lymphocytes that pass to spleen and lymph nodes and there settle down and mature into the populations of cells that look after the integrity and security of the body '. A second series of experiments on chickens confirmed some of the results of the first experiments, and clarified the nature of the function of the thymus. The results of both series of experiments are, in summary : In chickens, the thymus is not involved in ' antibody production as such, nor in cellular reactions against bacterial components. The cells involved in both of these are derived from the bursa. Perhaps the best suggestion is that the thymus, in both birds and mammals, liberates the cells whose descendants are primarily involved in the surveillance of cellular integrity in the body. When cell surfaces are changed by toxins or viruses or by simple aging . . . it is necessary for the survival and proper functioning of the individual that the anomalous cells should be recognized and dealt with. This may be the function of the cell populations that descend from the ancestral lymphocyte cells liberated from the thymus. In mammals it is highly probable that the thymus also carries out the function performed by the bursa of Fabricus in the chicken, which is to feed into the body the cells whose descendants will produce antibody.'

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[1] See C. G. Hempel, 'The Logic of Functional Analysis', in *Symposium on Sociological Theory*, ed. Llewellyn Gross, Row Peterson and Co., 1959, pp. 271-307 ; and A. Pap, *An Introduction to the Philosophy of Science*, Free Press, 1962, pp. 359-64

[2] The account is from M. Burnet, 'The Thymus Gland', *Scientific American*, November 1962, pp. 50-57.

2. In the example, what is explained is not the functioning of a thymus in some animal (e.g. in Fido) but rather the functioning of the thymus in certain classes of animals.

Secondly, the example indicates that we give a functional analysis of the thymus in, e.g. the mouse, if we state what the thymus does that is useful to the mouse.

If, e.g. we have shown that in the mouse the thymus produces lymphocyte cells which themselves have such and such useful effects, then we have shown that a function of the thymus is to produce these cells.

Let us generalise these remarks. Let '*S*' range over names of such classes as the mouse, the chicken, the vertebrate, and so on. Let '*I*' range over names of organs such as the heart, the thymus, or names of such classes as the red blood cell, the lymphocyte cell, or names of such parts of organisms as roots, leafs, cilia, and so on.[1] Assume we can recognise the linguistic form of a functional analysis. For example, both 'The function of the heartbeat is to circulate the blood' and 'The function of the heartbeat is to produce heart sounds' have this linguistic form; but the first states a function of the heart and the second does not. Then:

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[1] It is desirable also to let such processes as the heartbeat or the secretion of bile be included in the items for which a functional analysis is appropriate. Also it seems that '*S*' should include not only organisms (i.e. species and classes of such) but also such subsystems as, for example, the homeostasis of blood sugar (e.g. in man). For we can ask, e.g. what role does such and such play in keeping the sugar content of the blood constant? Further, it seems desirable to allow '*I*' and '*S*' to range not only over classes like the heart and the rabbit but over individuals as well. It is possible for a biologist to be concerned with the biology of some individual living thing. In the case of a mutation, for example, an organ might take on a new function, and this could be discovered and recorded. Also, allowing this broader range to '*I*' and '*S*' will perhaps facilitate a comparison of functional analysis in biology with functional analysis in the social sciences.

> *Preliminary account. FA* is a correct functional analysis of *I if and only if FA* has the linguistic form of a functional analysis, and *FA* states what *I* does in *S*, and what *I* does is useful to *S*.

In the account, 'does' is a determinable, we may say, where certain action verbs, as they may be called, are the determinants. Such verbs include, e.g. 'circulates', 'produces', 'stores', 'divides', provides', 'prevents' and so on.

3. The example of the thymus suggests that the above preliminary account is true. The following examples support this.

Speaking of the human body, Haldane and Huxley write: 'The liver, among its other functions, secretes bile.'[1] By the above account, this is a functional analysis since it states something the liver does, namely that it secretes bile, where what it does is useful to the body. The following examples are similar: 'The . . . function of the sodium chloride [in the plasma of mammals] is to prevent the cells from taking up too much water.'[2] 'A single cell in Hydra may serve for protection, be sensitive to external stimuli, contract when stimulated, pass on excitation to its neighbors, and perhaps secrete mucus, but in none of these ways will it act as efficiently as the various cells of a mammal, each of which performs one of these special functions.'[3]

In the Introduction to a book called *The Functions of the Blood*, the editors write: 'Though one hesitates to ask such a teleological question as "What is the blood for?" it is perfectly legitimate to ask "What does the blood do?"[4] The book contains selections on the functions of the blood designed to answer this question. One of the things the red blood cell does, we read, is that it provides 'a protective enzymic system that prevents irreversible oxidation of the ferrous pigment by the very oxygen it carries in the cell'.[5] Given this it is true that a function of the red cell is to provide this enzymic system.

Two final examples. 'The functional activity of antitoxins is to neutralize the killing effect of the [diphtheria] toxin.'[6] The 'common functional characteristic [of monocyte cells] is their ability to take in a variety of particulate material. . . . From the distribution of such cells we could predict that they played an important part in bodily defense against bacteria, and 60 or 70 years of experimental study has established their importance in this respect.'[1]

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[1] J. B. S. Haldane and J. Huxley, *Animal Biology*, Oxford University Press, 1927, p. 110 [2] Ibid. p. 158 [3] Ibid. p. 145

[4] Ed. R. G. Macfarlane, and A. H. T. Robb-Smith, Academic Press, 1961, p. vii

[5] Ibid. p. 99

[6] F. M. Burnett, *The Integrity of the Body*, Harvard University Press, 1962, p. 17

These examples are typical of functional analysis in biology. In each, the analysis states what the item whose function is in question does for the species or class having this item, although the species or class is often not mentioned explicitly in the analysis. And in each case, what the item does is useful to the members of the class. Thus the above preliminary account seems to state a sufficient condition for a statement's being a correct functional analysis. It seems that the account also states a necessary condition. If what the item of analysis does is not useful to the organisms of which it is a part, we do not say that doing this is one of the item's functions. For example, the sentence ' A function of the heartbeat in vertebrates is to produce heart sounds' is false because the production of heart sounds is not useful to vertebrates. Also, a functional analysis must say what the thing in question does. It must say, for example, that the thing *secretes*, or *provides*, *produces*, *prevents*, *enables*, and so on. Without such an action verb, we do not have a functional analysis.

2

1. The preliminary account states conditions which seem both necessary and sufficient for a sentence being a correct functional analysis. The problem remains of finding a translation schema which takes functional analyses into sentences that contain no teleological terms.

A view which has been tried, and which is plausible, is this. Sentences of the form

A function of I is to do C

mean

I does C.

This schema is not meant to provide a rule of transformation, but merely to indicate the kind of transformations that are to be made. For example, ' The function of the heart is to circulate the blood ' means, according to the schema, ' The heart circulates the blood '. The ' to do such and such ' phrase in the analysandum is changed into a ' does such and such' phrase, and we simply drop the phrase 'A function of'.

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[1] F. M. Burnett, *The Integrity of the Body*, Harvard University Press, 1962, p. 54

There are two objections to this view. Call each functional analysis an analysandum and its translation according to the above schema an analysans. For example, 'A function of the liver is to secrete bile' is an analysandum, 'The liver secretes bile' its analysans. Then, first, there are cases where the analysandum is false and its analysans true; and second, there are cases where the analysandum is true and its analysans false. We have already met an example of the first: the case of the heart producing heart sounds. Examples of the second arise when we say, as we would sometimes wish to, that such and such is a list of *all* the functions of some item. For example, consider the sentence, 'The following is a list of all the functions of the liver: it secretes bile, it . . ., and it stores sugar' (where the ellipsis is filled in). This sentence may be true, but the following sentence false: 'The following is a list of everything the liver *does*: It secretes bile, it . . . , and it stores sugar.' If the items on the list in the second case are all and only those on the list in the first case, as they should be by the above schema, then the sentence just indicated will be false; for the liver 'does' other things as well (just as one of the things the heart does is to produce heart sounds).

2. Consider an amended form of the above schema:

> *Preliminary translation schema.* A function of *I* (in *S*) is to do *C* *means* *I* does *C* and that *C* is done is useful to *S*.

For example '(In vertebrates) a function of the liver is to secrete bile' means 'The liver secretes bile, and that bile is secreted in vertebrates is useful to them'.

This schema does not encounter the above mentioned counterexamples. Further, if the preliminary account given in Part I is true, there will be no such counterexamples. There will be no cases of a true analysandum and false analysans, nor false analysandum and true analysans. For the preliminary schema incorporates the relevant necessary and sufficient conditions built into the preliminary account given in Part I. Since it seems true that if *I* does *C*, its doing *C* is one of its functions if and only if *C* is useful to *S*, we have an extensional equivalence of true functional analyses and true analysans, where the analysans are generated by, or are of the form of those generated by, the preliminary translation schema.

If, then, the word 'useful' which appears in each analysans can be replaced by words which are clearly non-teleological, there will be strong reason to believe that the teleological notions occurring in functional analyses can be dispensed with. Such a replacement is possible.

3. Let us restrict the discussion to the biology of plants and of animals other than man. Then it seems that what is done by an item, e.g. by the liver, is useful in one of two ways. Either what the item does is useful by aiding to preserve the life of things having the item, or else what it does is useful by aiding to preserve the species of things having it. For example, the secretion of bile is useful in the first way, the production of sperm in the second way.

Consider the sentence :

That C is done in S is useful to S.

This is, in effect, the second conjunct of the analysans provided by the preliminary translation schema. It seems that we can capture the two senses of ' useful ' in this sentence as follows. Let S_1 be a member of the species S, where it is true that the thing in question, namely C, is done in this member S_1 of S. Let S_2 be a member of S in which C is not done. For example, S_1 might be a mouse in which bile is secreted, S_2 a mouse in which bile is not secreted. Let $P(S_1, \text{sur})$ be the probability of S_1 continuing to live. ' Continuing to live ' should be supplemented by the specification of a time period. Assume an appropriate time period is specified in each case.

If it is true, *ceteris paribus*, that

$$P(S_1, \text{sur}) > P(S_2, \text{sur})$$

then that C is done is useful to S, in the first sense of useful.[1]

The *ceteris paribus* clause assumes that the two specimens, e.g. the two mice, are : (i) alike in other relevant respects (i.e. respects other than C), and (ii) in other respects normal. The *ceteris paribus* clause also assumes that the two specimens are in environments which are : (iii) in relevant respects the same in both cases, and (iv) normal. We may not know how to specify conditions which assure normality, but we can know if these conditions are met. For example, we can tell a normal mouse from a non-normal (e.g. a mutant) mouse. And just as we know that, normally if I drop a brick it will fall, so we can know that normally a mouse in which bile is not secreted will not have as good a chance of surviving as one in which bile is secreted. Our knowing these things does not seem to presuppose knowing the truth of any sentences containing teleological notions.

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[1] This idea was suggested to me by Keith S. Donnellan.

The second way in which an item is useful—i.e. by aiding in survival of the species—can, it seems, be captured as follows. Let S_1 and S_2 be as before, and let $P(S_1, \text{des})$ be the probability of S_1 having descendants. Then that C is done in S is useful to S in this second way if and only if it is true, *ceteris paribus*, that :

$$P(S_1, \text{des}) > P(S_2, \text{des})$$

By disjoining these two analysans it would seem that we get a necessary and sufficient condition for saying that a certain thing's being done is useful to a species or class.

There is, however, a further factor to be considered. Consider the following thought experiment. We have two bottle capping machines, which are alike except that one has a non-functioning part the other lacks : a fifth wheel, say, which emits a screeching noise when the machine is running. It is conceivable that the machine with the redundant part successfully caps more bottles per hour than the other machine ; and in fact conceivable that this chance result occur in all such pairs of machines. Now by analogy with the above translation schema we would have to say that a function of the fifth wheel is to emit screeching sounds ; whereas in fact the wheel performs no function.

This example helps us see that the following inference is faulty. 'I does C and, *ceteris paribus*, $P(S_1, \text{sur}) > P(S_2, \text{sur})$' implies 'A function of I is to C'. The inference is faulty because it may be a mere matter of chance, as in the machine case, that animals with C have a greater chance of surviving than those without C. Yet the above translation schema warrants this inference.

We can meet the difficulty by introducing a subjunctive conditional into the translation schema. We will say :

> *Translation schema.* A function of I (in S) is to do C *means* I does C ; and if, *ceteris paribus*, C were not done in an S, then the probability of that S surviving or having descendants would be smaller than the probability of an S in which C is done surviving or having descendants.

The subjunctive conditional formulation excludes cases where it is a mere accident, or matter of chance, that the probability of survival or of having descendants is greater. This is evident in the example of the machines. It may be that machines with screeching fifth wheels cap more bottles ; but it does not follow that if a similar machine *were* to have a fifth wheel which screeches, it *would* cap more bottles.

4. In summary, the translation schema offered above is supported by this claim : The schema takes all statements which have the linguistic

form of a functional analysis and which are true, into true statements, and all false statements of this form into false statements. Further, this holds in the opposite direction (when the range of '*I*' and '*S*' is appropriately restricted).

The claim, in turn, is supported, first, by the plausibility of the preliminary account in Part 1, and second by the seeming correctness of the above discussion of 'useful'.

If the claim is correct, this does not guarantee the correctness of the translation schema, for the schema claims to generate statements synonymous with the original functional analyses (or perhaps *analyses* of these). But I do not see what an analysans generated by the schema fails to say that is said in the corresponding analysandum, or vice versa.

If the translation schema is correct, it seems correct to say that the teleological words such as 'function' used in biology can be dispensed with.

3

As has been noted, functional analyses serve as *explanations*. Someone might say, 'Explain the function of the thymus', or ask, 'What is the function of the thymus?' or 'Why do animals have a thymus?' When we answer, 'The function of the thymus is [such and such]' we have, it seems plain, given an explanation.

It has been held that functional analyses qua explanation (or 'teleological explanations') have the same form as deductive nomological explanations.[1]

This position has been stated very clearly by Arthur Pap, who considers a variant of the simple example mentioned in the introduction: 'The heart beats in order to circulate the blood.' 'The heart's activity', Pap notes, 'is here explained in terms of the function it serves'.[2] He brings out the explanatory force of the teleological explanation as follows: 'The organism cannot survive unless the blood circulates, and the blood cannot circulate unless the heart beats: that's why the heart beats.'[3] He adds:

> It is easy to show that in respect of their logical form and empirical conditions of adequacy (teleological explanations) do not differ from

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[1] This is the familiar 'covering law model' of scientific explanation. See C. G. Hempel and Paul Oppenheim, 'Studies in the Logic of Explanation', *Philosophy of Science*, 1948, 15, 135-175; and K. R. Popper, *The Logic of Scientific Discovery*, New York, 1959, Sect. 12.

[2] Op cit. p. 360

[3] Ibid.

> causal explanations. To assert that the beating of the heart is a necessary condition for blood circulation is equivalent to asserting that blood circulation is a sufficient condition for the beating of the heart. And this is to assert the confirmable lawlike generalization that if blood circulates in an organism, then the organism contains a beating heart. Add the verifiable premise that blood circulates in this organism and you get deductively the conclusion that this organism contains a beating heart.[1]

The position in question, then, is that in biology teleological explanations fit the covering law model of explanation. In the example of the heart we have :

1. The blood circulates only if the heart beats.
2. The blood circulates in Fido (i.e. in this organism).

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3. Therefore, Fido contains a beating heart.[2]

(1) is a ' confirmable lawlike generalisation ', (2) a statement of antecedent conditions, and (3) follows deductively from (1) and (2). The original teleological explanation is to be unpacked and supplemented as in (1) and (2) ; when this is done we see that it has the form of a deductive nomological explanation.

This view of teleological explanation is wrong. This becomes clear when we notice the disparity between the question which originally provokes a teleological explanation, and the answer given by the above set of premises. The question is : 'Why does the heart beat?' (Note Pap's ' That's why the heart beats '.) Whereas the above set of premises answers a different question, namely, 'Why does Fido (this organism) contain a beating heart?'

This point may be put as follows. On the covering law model, what follows from the premises is the explanandum—i.e. what is being explained. The premises are the explanans—i.e. the explanation. Thus, ' The function of the heart is to circulate the blood ' is, when unpacked (and in the above view, supplemented), the explanation, and what follows is what is being explained : namely, that Fido has a beating heart. But surely the latter is not in question. When we ask for a functional analysis of some item, such as the heartbeat, or the thymus, it is not at all problematic that the thymus, say, is *there*, or is *present*. What is problematic is what it is there *for*—i.e. what it does. It is the latter question, the question, ' what does the thymus do (that is useful)? ' that we attempt to answer with a teleological explanation. If all the explanation told us was that this animal, or for that matter a certain class of animals, *has* a thymus, we would consider our question evaded, not answered.

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[1] Op. cit. p. 360

[2] In ' The Logic of Functional Analysis ', loc. cit., Carl Hempel offers a similar account of functional analysis *qua* explanation. Hempel, however, believes that in most cases the sentence corresponding to the ' only if ' statement which constitutes premise 1 above will be, as a matter of empirical fact, false. (A point disputed by Nagel, op. cit. p. 284.) From this alleged fact, he concludes that functional analysis *qua* explanation usually fails to explain what it attempts to explain. He writes, for example, ' The information typically provided by a functional analysis of an item *i* affords neither deductively nor inductively adequate grounds for expecting *i* rather than one of its alternatives. The impression that a functional analysis does provide such grounds, and thus explains the occurrence of *i*, is no doubt at least partly due to the benefit of hindsight (p. 286).'

The confusion arises, perhaps, because the why-question we might use in asking for a teleological explanation is ambiguous. Thus the question 'Why do animals have livers?' might be interpreted as a request for a covering law explanation of why livers are present in animals. On the other hand, it might be interpreted as asking what the liver does, or what it is good for. When we give a teleological explanation in answer to the question, we have interpreted it in the second and not the first way. This is obvious from the examples cited earlier, and from the analysis of them in terms of the translation schema offered above.

Thus in biology so called teleological explanations are of a different kind from explanations which fit the covering law model. The latter attempt to *account* for something's being present, or having occurred, by subsuming it under a general law, and by citing appropriate ' antecedent conditions '. Teleological explanations in biology, of the kind we have been considering, do no such thing. They merely state what the thing in question does that is useful to the organisms that have it. Since the functional analyses which constitute the kind of teleological explanations we have been considering can be translated into non-teleological sentences, such teleological explanations are misnamed.

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PROOFS AND REFUTATIONS (IV) *

I. LAKATOS

§ 7. The Problem of Content Revisited.

- (*a*) The naiveté of the naive conjecture.
- (*b*) Induction as the basis of the method of proofs and refutations.
- (*c*) Deductive guessing versus naive guessing.
- (*d*) Increasing content by deductive guessing.
- (*e*) Logical versus heuristic counterexamples.

§ 8. Concept-formation.

- (*a*) Refutation by concept-stretching. A reappraisal of monster-barring—and of the concepts of error and refutation.
- (*b*) Proof-generated versus naive concepts. Theoretical versus naive classification.
- (*c*) Logical and heuristic refutations revisited.
- (*d*) Theoretical versus naive concept-stretching. Continuous versus critical growth.

§ 9. How Criticism may turn Mathematical Truth into Logical Truth.

- (*a*) Unlimited concept-stretching destroys meaning and truth.
- (*b*) Mitigated concept-stretching may turn mathematical truth into logical truth.

7 *The Problem of Content Revisited*

(*a*) *The naiveté of the naive conjecture*

ZETA: I agree with Omega in deploring the fact that monster-barrers, exceptionbarrers and lemma-incorporators all strove for certain truth at the expense of content. But his *Rule 4*,[1] demanding deeper proofs of the same naive conjecture, is not enough. Why should our search for content be delimited by the first naive conjecture we stumble upon? Why should the aim of our enquiry be the 'domain of the naive conjecture'?

OMEGA: I don't follow you. Surely our problem was to discover the domain of truth of $V-E+F=2$?

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* Parts I, II and III appeared in the preceding numbers.

[1] See Part III, p. 237

ZETA: It was not! Our problem was to find out the relation between V, E and F for any polyhedron whatsoever. It was a sheer accident that we first got familiar with polyhedra for which $V-E+F = 2$. But a critical inquiry into these 'Eulerian' polyhedra showed us that there are many more non-Eulerian than Eulerian polyhedra. Why not look for the domain of $V-E+F = -6$, $V-E+F = 28$ or $V-E+F = 0$? Aren't they equally interesting?

SIGMA: You are right. We paid so much attention to $V-E+F = 2$ only because we originally thought it was true. Now we know it is not—we have to find *a new, deeper naive conjecture* . . .

ZETA: . . . that will be less naive. . . .

SIGMA: . . . that will be a relation between V, E, and F for *any* polyhedron.

OMEGA: Why rush? Let us first solve the more modest problem that we set out to solve: to explain why some polyhedra are Eulerian. Until now we have arrived only at partial explanations. For instance, none of the proofs found has explained why a picture-frame with ringshaped faces both in the front and in the back is Eulerian (Fig. 16). It has 16 vertices, 24 edges and 8 faces. . . .

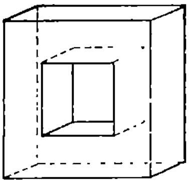

FIG. 16

THETA: It is certainly not a Cauchy-polyhedron: it has a tunnel, it has ringshaped faces. . . .

BETA: And yet Eulerian! How irrational! Is a polyhedron guilty of a single fault—a tunnel without ringshaped faces (Fig. 9)—to be cast out among the goats, yet one which offends in twice as many ways—having also ringshaped faces (Fig. 16)—admitted to the sheep?[1]

OMEGA: You see, Zeta, we have enough puzzles about Eulerian polyhedra. Let us solve them before we go on to a more general problem.

ZETA: No, Omega. 'More questions may be easier to answer than just one question. A new more ambitious problem may be easier to handle than the original problem.'[2] Indeed, I shall show you that your narrow, accidental problem can only be solved by solving the wider, essential problem.

OMEGA: But I want to discover the secret of Eulerianness!

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[1] The problem was noticed by Lhuilier ([1812-13], p. 189) and, independently, by Hessel [1832]. In Hessel's paper the figures of the two picture-frames appear next to each other. Also cf. p. 309 footnote 1.

[2] Pólya calls this the 'inventor's paradox' ([1945], p. 110).

ZETA: I understand your resistance. You have fallen in love with the problem of finding out where God drew the firmament dividing Eulerian from non-Eulerian polyhedra. But there is no reason to believe that the term 'Eulerian' occurred in God's blueprint of the universe at all. What if Eulerianness is merely an accidental property of some polyhedra? In this case it would be uninteresting or even impossible to find out the random zigzags of the demarcation line between Eulerian and non-Eulerian polyhedra. Such an admission however would leave rationalism unsullied, for Eulerianness is then not part of the rational design of the universe. So let us forget about it. One of the main points about critical rationalism is that one is always prepared to abandon one's original problem in the course of the solution and replace it by another one.

(*b*) *Induction as the basis of the method of proofs and refutations*

SIGMA: Zeta is right. What a disaster!

ZETA: Disaster?

SIGMA: Yes. You now want a new 'naive conjecture' about the relation between V, E and F, for *any* polyhedron, don't you? Impossible! Look at the vast crowd of counterexamples. Polyhedra with cavities, polyhedra with ringshaped faces, with tunnels, joined together at edges, vertices . . . $V-E+F$ can take any value whatsoever! You cannot possibly recognise any order in this chaos! We have left the firm ground of Eulerian polyhedra for a swamp! We have irretrievably lost a naive conjecture and have no hope of getting another one!

ZETA: But . . .

BETA: Why not? Remember the seemingly hopeless chaos in our table of the numbers of vertices, edges and faces even of the most ordinary convex polyhedra:

	Polyhedron	F	V	E
I	cube	6	8	12
II	triangular prism	5	6	9
III	pentagonal prism	7	10	15
IV	square pyramid	5	5	8
V	triangular pyramid	4	4	6
VI	pentagonal pyramid	6	6	10
VII	octahedron	8	6	12
VIII	'tower'	9	9	16
IX	'truncated cube'	7	10	15

We failed so many times to fit them into a formula.[1] But then suddenly the real regularity governing them struck us: $V-E+F=2$.

KAPPA [*aside*]: 'Real regularity'? Funny expression for an utter falsehood.

BETA: All that we have to do now is to complete our table with the data for non-Eulerian polyhedra and look for a new formula: with patient, diligent observation, and some luck, we shall hit on the right one; then we can improve it again by applying the method of proofs and refutations!

ZETA: Patient, diligent observation? Trying one formula after the other? Perhaps you will devise a guessing machine that produces random formulas and tests them against your table? Is this your idea of how science progresses?

BETA: I don't understand your scorn. Surely you agree that our first knowledge, our naive conjectures, can only come from diligent observation and sudden insight, however much our critical method of 'proofs and refutations' takes over once we have *found* a naive conjecture? Any deductive method has to start from an inductive basis!

SIGMA: Your inductive method will never succeed. We only arrived at $V-E+F=2$ because there happened to be no picture-frame or urchin in our original tables. Now that this historical accident. . . .

KAPPA [*aside*]: . . . or God's benevolent guidance. . . .

SIGMA: . . . is no more, you will never 'induce' order from chaos. We started with long observation and lucky insight—and failed. Now you propose to start again with longer observation and luckier insight. Even if we did arrive at a new naive conjecture —which I doubt—we shall only end up in the same mess.

BETA: Perhaps we should give up research altogether? We *have* to start again—first with a new naive conjecture and then going again through the method of proofs and refutations.

ZETA: No, Beta. I agree with Sigma—therefore I shall not start again with a new naive conjecture.

BETA: Then where do you want to *start* if not with an inductive low-level generalisation as a naive conjecture? Or have you an alternative method for starting?

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[1] See footnote 3, p. 303. The table has been borrowed from Pólya [1954], Vol. I, p. 36.

(c) *Deductive guessing versus naive guessing*

ZETA: Start? Why should I *start*? My mind is not empty when I discover (or invent) a problem.

TEACHER: Do not tease Beta. Here is the problem: '*Is there a relation between the number of vertices, edges and faces of polyhedra analogous to the trivial relation between the number of vertices and edges of polygons, namely that* V = E?'[1] How would *you* set about it?

ZETA: First, I have no government grants to conduct an extensive survey of polyhedra, no army of research assistants counting the numbers of their vertices, edges and faces and compiling tables from the data. But even if I had, I should have no patience—or interest—in trying one formula after the other to test whether it fits.

BETA: What then? Will you lie down on your couch, shut your eyes and forget about the data?

ZETA: Exactly. I need an *idea* to start with, but no data whatsoever.

BETA: And where do you get your idea from?

ZETA: It is already there in our minds when we formulate the problem: in fact, it is in the very formulation of the problem.

BETA: What idea?

ZETA: That for a polygon $V = E$.

BETA: So what?

ZETA: A problem never comes out of the blue. It is always related to our background knowledge. We know that for polygons $V = E$. Now a polygon is a system of polygons consisting of one single polygon. A polyhedron is a system of polygons consisting of more than a single polygon. But for polyhedra $V \neq E$. At what point did the relation $V = E$ break down in the transition from monopolygonal systems to polypolygonal systems? Instead of collecting data I trace how the problem grew out of our background knowledge; or, which was the expectation whose refutation presented the problem?

SIGMA: Right. Let us follow your recommendation. For any polygon $E - V = 0$ (Fig. 17*a*). What happens if I fit another polygon to it (not necessarily in the same plane)? The additional polygon has n_1 edges and n_1 vertices; now by fitting it to the original one along a chain of n_1' edges and $n_1'+1$ vertices we shall increase the number of edges by $n_1 - n_1'$ and the number of vertices by $n_1 - (n_1'+1)$; that is, in the new 2-polygonal system there will be an excess in the number of edges over the number of vertices: $E-V=1$ (Fig. 17*b*; for an unusual but perfectly proper fitting see Fig. 17*c*). 'Fitting' a new face to the system will always increase this excess by one, or, for an F-polygonal system constructed in this way $E-V=F-1$.

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[1] See Part I, p. 7

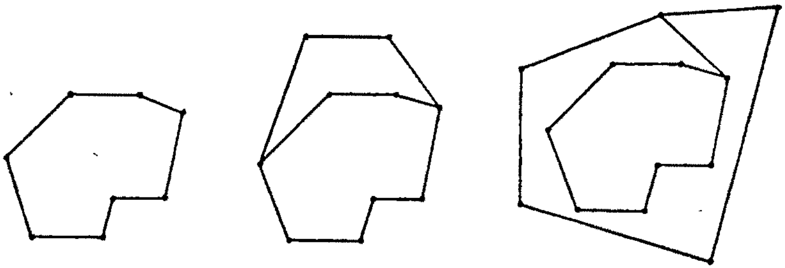
(a) (b) (c)

Fig. 17

Zeta: Or, $V-E+F=1$.

Lambda: But this is false for most polygonal systems. Take a cube. . . .

Sigma: But my construction can lead only to 'open' polygonal systems—bounded by a circuit of edges! I can easily extend my thought-experiment to 'closed' polygonal systems, with no such boundary. Such closure can be accomplished by covering an open vase-like polygonal system with a polygon-cover: fitting such a covering polygon will increase F by one without changing V or E. . . .

Zeta: Or, for a closed polygonal system—or closed polyhedron—constructed in this way, $V-E+F+2$: a conjecture which now you have got without 'observing' the number of vertices, edges and faces of a single polyhedron!

Lambda: And now you can apply the method of proofs and refutations without an 'inductive starting point'.

Zeta: With the difference that you do not need to devise a proof—the proof is already there! You can go on immediately with refutations, proof-analysis, theorem-formation.

Lambda: Then in your method—instead of observations—proof precedes the naive conjecture![1]

Zeta: Well, I shouldn't call a conjecture that has grown out of a proof 'naive'. In my method there is no place for inductive naiveties.

Beta: Objection! You only pushed back the 'naive' inductive start: you start with '$V=E$ for polygons'. Don't you base this on observations?

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[1] This is an important qualification to footnote 2, p. 10 in Part I.

ZETA: Like most mathematicians, I cannot count. I just tried to count the edges and vertices of a heptagon: I found first 7 edges and 8 vertices, and then again 8 edges and 7 vertices. . . .

BETA: Joking apart, how *did* you get $V = E$?

ZETA: I was deeply shocked when I first realised that for a triangle $V - E = 0$. I knew of course very well that in an edge $V - E = 1$ (Fig. 18*a*). I also knew that fitting new edges will always result in an increase by one, both in the number of vertices and edges (Figs. 18*b* and 18*c*). Why, in polygonal edge-systems, does $V - E = 0$? Then I realised that this is because of the transition from an open system of edges (which is bounded by two vertices) to a closed system of edges (which has no such boundary): because we 'cover' the open system up by fitting an edge without adding a new vertex. So I proved, not observed, that $V - E = 0$ for polygons.

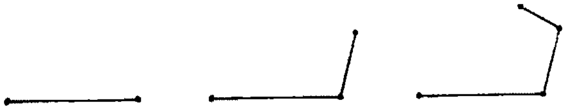
(a) (b) (c)

FIG. 18

FIG. 19

BETA: Your ingenuity will not help you. You only pushed back the inductive starting point further: now to the statement that $V - E = 1$ for any edge whatsoever. Did you prove or did you *observe* that?

ZETA: I proved it. I knew of course that for a single vertex $V = 1$ (Fig. 19). My problem was to construct an analogous relation. . . .

BETA [*furious*]: Didn't you *observe* that for a point $V = 1$?

ZETA: Did *you*? [*Aside, to Pi*]: Should I tell him that my 'inductive starting point' was empty space? That I began by 'observing' *nothing*?

LAMBDA: Whatever the case, two points have been made. First Sigma argued that *it is due only to historical accidents that one can arrive at naive inductive conjectures*: when one is faced with a real chaos of facts, one will scarcely be able to fit them into a nice formula. Then Zeta showed that *for the logic of proofs and refutations we need no naive conjecture, no inductivist starting point at all.*

BETA: Objection! What about those celebrated conjectures that have *not* been preceded (or even followed) by proofs, such as the four-colour conjecture that says that four colours are enough to colour any map, or the Goldbach conjecture? It is only by historical accidents that proofs can precede theorems, that Zeta's 'deductive guessing' can take place: otherwise naive inductive conjectures come first.

TEACHER: We certainly have to learn *both* heuristic patterns: *deductive guessing* is best, but *naive guessing* is better than no guessing at all. But *naive guessing is not induction: there are no such things as inductive conjectures*!

BETA: But we found the naive conjecture by *induction*! 'That is, it was suggested by observation, indicated by particular instances. . . . And among the particular cases that we have examined we could distinguish two groups: those which preceded the formulation of the conjecture and those which came afterwards. The former *suggested* the conjecture, the latter *supported* it. Both kinds of cases provide some sort of contact between the conjecture and "the facts". . . .'[1] This double contact is the heart of induction: the first makes *inductive heuristic*, the second makes inductive justification, or *inductive logic*.

TEACHER: No! Facts do not suggest conjectures and do not support them either!

BETA: Then what suggested $V-E+F=2$ to *me*, if not the facts, listed in my table?

TEACHER: I shall tell you. You yourself said you failed many times to fit them into a formula.[2] Now what happened was this: you had three or four conjectures which in turn were quickly refuted. Your table was built up in the process of testing and refuting these conjectures. These dead and now forgotten conjectures suggested the facts, not the facts the conjectures. *Naive conjectures are not inductive conjectures: we arrive at them by trial and error, through conjectures and refutations.*[3] But if you—wrongly—believe that you arrived at them inductively, from your tables, if you believe that the longer the table, the more conjectures it will suggest, and later support, you may waste your time compiling unnecessary data. Also, being indoctrinated that the path of discovery is from facts to conjecture, and from conjecture to proof (the myth of induction), you may completely forget about the heuristic alternative: deductive guessing.[1]

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[1] Pólya [1954], Vol. I, pp. 5 and 7

[2] See p. 299

[3] These trials and errors are beautifully reconstructed by Pólya. The first conjecture is that F increases with V. This being refuted, two more conjectures follow: E increases with F; E increases with V. The fourth is the winning guess: $F+V$ increases with E ([1954], Vol. I, pp. 35-37).

Mathematical heuristic is very like scientific heuristic—not because both are inductive, but because both are characterised by conjectures, proofs, and refutations. The—important—difference lies in the nature of the respective conjectures, proofs (or, in science, explanations), and counterexamples.[2]

BETA: I see. Then our naive conjecture was not the *first* conjecture ever, 'suggested' by hard, non-conjectural facts: it was preceded by many 'pre-naive' conjectures and refutations. The logic of conjectures and refutations has no starting point—but the logic of proofs and refutations has: it starts with the first naive conjecture to be followed by a thoughtexperiment.

ALPHA: Perhaps. But then I should not have called it 'naive'![3]

KAPPA [*aside*]: Even in heuristic there is no such thing as perfect *naiveté*!

BETA: The main thing is to get out of the trial-and-error period as soon as possible, to proceed quickly to thoughtexperiments without having too much 'inductive' respect for 'facts'. Such respect may hamper the growth of knowledge. Imagine that you arrive by trial-and-error at the conjecture: $V-E+F=2$, and that it is immediately refuted by the observation that $V-E+F=0$ for the picture-frame. If you have too much respect for facts, especially when they refute your conjectures, you will go on with pre-naive trial-and-error and look for another conjecture. But if you have a better heuristic, you at least *try* to ignore the adverse observational test, and try a *test by thoughtexperiment*: like Cauchy's proof.

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[1] On the other hand those who, because of the usual deductive presentation of mathematics, come to believe that the path of discovery is from axioms and/or definitions to proofs and theorems, may completely forget about the possibility and importance of naive guessing. In fact in mathematical heuristic it is deductivism which is the greater danger, while in scientific heuristic it is inductivism.

[2] We owe the revival of mathematical heuristic in this century to Pólya. His stress on the similarities between scientific and mathematical heuristic is one of the main features of his admirable work. What may be considered his only weakness is connected with this strength: he never questioned that science is inductive, and because of his correct vision of deep analogy between scientific and mathematical heuristic he was led to think that mathematics is also inductive. The same thing happened earlier to Poincaré (see his [1902], Introduction) and also to Fréchet (see his [1938]).

[3] See Part II, p. 138

SIGMA: What confusion! Why call Cauchy's *proof* a *test*?

BETA: Why call Cauchy's *test* a *proof*? It was a *test*! Listen. You started with a naive conjecture: $V-E+F=2$ for all polyhedra. Then you drew consequences from it: 'if the naive conjecture is true, after removing a face, for the remaining network $V-E+F=1$'; 'if this consequence is true, $V-E+F=1$ even after triangulation'; 'if this last consequence is true, $V-E+F=1$ will hold while triangles are removed one by one'; 'if this is true, $V-E+F=1$ for one single triangle'. . . .

Now this last conclusion happens to be known to be true. But what if we had concluded that for a single triangle $V-E+F=0$? We would immediately have rejected the original conjecture as false. All that we have done is to test our conjecture: to draw consequences from it. The test seemed to corroborate the conjecture. But corroboration is not proof.

SIGMA: But then our proof proved even less than we thought it did! We then have to reverse the process and try to construct a thoughtexperiment which leads in the opposite direction: from the triangle back to the polyhedron!

BETA: That is right. Only Zeta pointed out that instead of solving our problem by first devising a naive conjecture through trial and error, then testing it, then reversing the test into a proof, we can start straight away with the real proof. Had we realised the possibility of deductive guessing we might have avoided all this pseudo-inductive fumbling!

KAPPA [*aside*]: What a dramatic series of *volte-faces*! Critical Alpha has turned into a dogmatist, dogmatist Delta into a refutationist, and now inductivist Beta into a deductivist!

SIGMA: But wait. If the *test-thoughtexperiment*. . . .

BETA: I shall call it *analysis*. . . .

SIGMA: . . . can be followed up at all by a *proof-thoughtexperiment*. . . .

BETA: I shall call it *synthesis*. . . .[1]

SIGMA: . . . will the 'analytic theorem' be necessarily identical with the 'synthetic theorem'? In going in the opposite direction we might use different lemmas![2]

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[1] According to Pappian heuristic, mathematical discovery starts with a conjecture, which is followed by *analysis* and then, provided *analysis* does not falsify the conjecture, by *synthesis*. (Also cf. Part I, p. 10, footnote 2, and Part III, p. 243, footnote 1.) But while our version of *analysis-synthesis improves* the conjecture, the Pappian version only *proves* or *disproves* it.

[2] Cf. Robinson [1936], p. 471

Beta: If they are different, then the synthetic theorem should supersede the analytic one—after all analysis only *tests* while synthesis *proves*.

Teacher: Your discovery that our '*proof*' was in fact a *test* seems to have shocked the class and diverted their attention from your main argument: that if we have a conjecture that has already been refuted by a counterexample, we should push the refutation aside and try to test the conjecture by a thoughtexperiment: this way, we might hit on a proof, leave the phase of trial-and-error, and switch to the method of proofs and refutations. But it was exactly this which made me say that 'I am willing to set out to "prove" a false conjecture'![1] And Lambda too demanded in his *Rule 1*: 'If you have a conjecture set out to prove it *and* refute it.'

Zeta: That is right. But let me supplement Lambda's rules and Omega's *Rule 4* by

> *Rule 5. If you have counterexamples of any type, try to find, by deductive guessing, a deeper theorem to which they are not counterexamples any longer.*

Omega: You now stretch my concept of 'depth'—and you may be right. But what about the actual application of your new rule? Until now it has only given us results that we already knew. It is easy to be wise after the event. Your 'deductive guessing' is just the *synthesis* corresponding to Teacher's original *analysis*. But now you should be honest—you must use your method to find a conjecture which you do not already know about, with the promised increase in content.

Zeta: Right. I start with the theorem generated by *my* thought-experiment: '*All closed normal polyhedra are Eulerian.*'

Omega: 'Normal'?

Zeta: I don't want to waste time going through the method of proof and refutations. I just call 'normal' all polyhedra that can be built up from a 'perfect' polygon by fitting to it (*a*) first $F-2$ faces without changing $V-E+F$ (these will be *open* normal polyhedra) and (*b*) then a last closing face which increases $V-E+F$ by 1 (and turns the *open* polyhedron into a *closed* one).

Omega: 'Perfect polygon'?

Zeta: By a 'perfect' polygon I mean one that can be built up from one single vertex by fitting to it first $n-1$ edges without changing $V-E$, and then a last closing edge which decreases $V-E$ by 1.

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[1] See Part I, p. 25

Omega: Will your closed normal polyhedra coincide with our Cauchy polyhedra?

Zeta: I do not want to go into that now.

(*d*) *Increasing content by deductive guessing*

Teacher: Enough of preliminaries. Let us see your deduction.

Zeta: Yes, Sir. I take two closed normal polyhedra (Fig. 20*a*) and paste them together along a polygonal circuit so that the two faces that meet disappear (Fig. 20*b*). Since for the two polyhedra $V-E+F=4$, the disappearance of two faces in the united polyhedron will just restore the Euler formula—no surprise after Cauchy's proof since the new polyhedron can also easily be pumped into a ball. So the formula stands up well to this pasting test. But let us now try a double-pasting test: let us 'paste' the two polyhedra together along two polygonal circuits (Fig. 20*c*). Now 4 faces will disappear and for the new polyhedron $V-E+F=0$.

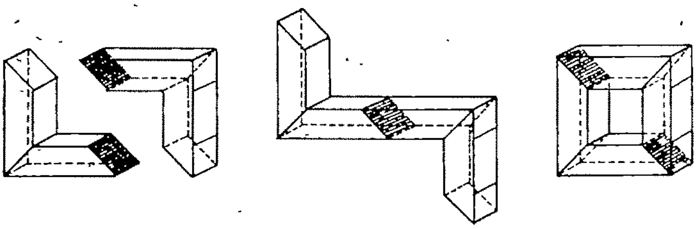
(a) (b) (c)

Fig. 20

Gamma: This is Alpha's *Counterexample 4*, the picture-frame!

Zeta: Now if I 'double-paste' to this picture-frame (Fig. 20*c*) yet another normal polyhedron (Fig. 21*a*), $V-E+F$ will be -2 (Fig. 21*b*). . . .

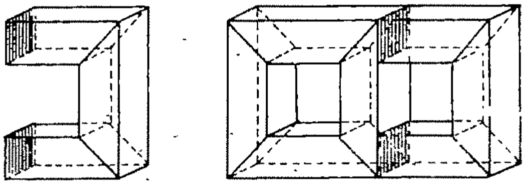
(a) (b)

Fig. 21

Sigma: For a monospheroid polyhedron $V-E+F=2$, for a dispheroid polyhedron $V-E+F=0$, for a trispheroid $V-E+F=-2$, for an n-spheroid polyhedron $V-E+F=2-2(n-1)$. . . .

ZETA: . . . which is your new conjecture of unprecedented content, complete with proof, without having compiled a single table.[1]

SIGMA: This is really nice. Not only did you explain the obstinate picture-frame, but you produced an infinite variety of novel counterexamples. . . .

ZETA: Complete with explanation.

RHO: I just arrived at the same result in a different way. Zeta started with two Eulerian examples and turned them into a counterexample in a controlled experiment. I start with a counterexample and turn it into an example. I made the following thoughtexperiment with a picture-frame: 'Let the polyhedron be of some stuff that is easy to cut like soft clay, let a thread be pulled through the tunnel and then through the clay. It will not fall apart. . . .'[2] But it has become a familiar, simple, spheroid polyhedron! It is true, we increase the number of faces by 2, and the numbers of both edges and vertices by m; but since we know that the Euler characteristic of a simple polyhedron is 2, the original must have had the characteristic 0. Now if one needs more, say n, such cuts to reduce the polyhedron to a simple one, its characteristic will be $2-2n$.

SIGMA: This is interesting. Zeta has already shown us that we may not need a conjecture in order to start *proving*, that we may immediately devise a *synthesis*, i.e. a proof-thoughtexperiment from a related proposition that is known to be true. Now Rho shows that we may not need a conjecture even in order to start *testing*, but we may set out—*pretending* that the result is already there—to devise an *analysis*, i.e. a test-thoughtexperiment.[3]

OMEGA: But whichever way you choose, you still leave hordes of polyhedra unexplained! According to your new theorem for all polyhedra $V-E+F$ is an even number, less than 2. But we saw quite a few polyhedra with *odd* Euler characteristics. Take the crested cube (Fig. 12) with $V-E+F=1$. . . .

ZETA: I never said that my theorem applies to *all* polyhedra. It applies only to all n-spheroid polyhedra built up according to my construction. My construction as it stands does not lead to ringshaped faces.

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[1] This was done by Raschig [1891].

[2] Hoppe [1879], p. 102

[3] This is again part of Pappian heuristic. He calls an *analysis* starting with a conjecture '*theoretical*', and an analysis starting with no conjecture '*problematical*' (Heath [1925], Vol. I, p. 138). The first refers to *problems to prove*, the second to *problems to solve* (or *problems to find*). Also cf. Pólya [1945], pp. 129-136 ('Pappus') and 197-204 ('Working backwards').

OMEGA: So?

SIGMA: I know! One can also extend it to polyhedra with ringshaped faces: one may construct a ringshaped polygon by deleting in a suitable proof-generated system of polygons an edge without reducing the number of faces (Figs. 22*a* and 22*b*). I wonder, perhaps there are

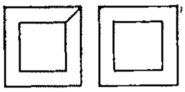
(a) (b)

FIG. 22

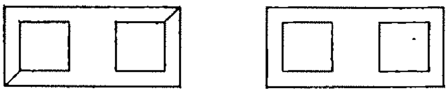
(a) (b)

FIG. 23

also 'normal' systems of polygons, constructed in accordance with our proof, in which we can delete even more than one edge without reducing the number of faces. . . .

GAMMA: That is true. Look at this 'normal' polygonal system (Fig. 23*a*). You can delete two edges without reducing the number of faces (Fig. 23*b*).

SIGMA: Good! Then in general

$$V-E+F=2-2(n-1)+\sum_{k=1}^{F} e_k$$

for *n*-spheroid—or *n*-tuply connected—polyhedra with e_k edges deleted without reduction in the number of faces.

BETA: This formula explains my crested cube (Fig. 12), a monospheroid polyhedron ($n=1$) with one ringshaped face: e_k are zero, except for e_6 which is 1, or $\sum_{k=1}^{F} e_k=1$, consequently $V-E+F=1$.

SIGMA: It also explains your 'irrational' Eulerian freak: the cube with two ringshaped faces and one tunnel (Fig. 16). It is a dispheroid polyhedron ($n=2$) with $\sum_{k=1}^{F} e_k=2$. Consequently its characteristic is $V-E+F=2-2+2=2$. Moral order is restored to the world of polyhedra![1]

OMEGA: What about polyhedra with cavities?

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[1] The 'order' was restored by Lhuilier with approximately the same formula ([1812-13], p. 189); and by Hessel with clumsy *ad hoc* formulas about different ways of fitting Eulerian polyhedra together ([1832], pp. 19-20). Cf. p. 297, footnote 1.

SIGMA: I know! For them one has to add up the Euler characteristics of each disconnected surface:

$$V-E+F=\sum_{j=1}^{K}\left\{2-2(n-1)+\sum_{k=1}^{F}e_k\right\}.$$[1]

BETA: And the twin-tetrahedra?

SIGMA: I know! . . .

GAMMA: What is the use of all this precision? Stop this flood of pretentious trivialities![2]

ALPHA: Why should he? Or are the twin-tetrahedra monsters, not genuine polyhedra? A twin-tetrahedron is just as good a polyhedron as your cylinder! But you liked *linguistic* precision.[3] Why do you deride our new precision? We have to make the theorem cover *all* polyhedra—by making it precise we are increasing its content, not decreasing it. This time precision is a virtue!

KAPPA: Boring virtues are just as bad as boring vices! Besides, you will never achieve *complete* precision. We should stop when it ceases to be interesting to go on.

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[1] Historically Lhuilier—in his [1812–13]—managed to generalise Euler's formula by naive guessing and arrived at the following formula: $V-E+F=2[(C-T+1)+(p_1+p_2+\ .\ .\ .)]$, where C is the number of cavities, T the number of tunnels and p_1 the number of inner polygons on the *i*th face. He also *proved* it as far as 'inner polygons' were concerned, but tunnels seem to have defeated him. He constructed the formula in an attempt to account for his three kinds of 'exceptions'; but his list of exceptions was incomplete. (Cf. Part II, p. 123, footnote 1.) Moreover, this incompleteness was not the only reason for the falsity of his naive conjecture: for he did not notice the possibility that cavities might be multiply-connected; that one may not be able to determine unambiguously the number of tunnels in polyhedra with a system of branching tunnels; and that it is not 'the number of inner polygons', but the number of ringshaped faces that is relevant (his formula breaks down for two adjacent inner polygons, with an edge in common). For a criticism of Lhuilier's 'inductive generalisation' see Listing [1861], pp. 98–99. Also cf. p. 322, footnote 2.

[2] Quite a few mathematicians of the nineteenth century were confused by such trivial increases in content, and did not really know how to deal with them. Some—like Möbius—used monster-barring definitions (see Part I, p. 17); others—like Hoppe—monster-adjustment. Hoppe's [1879] is particularly revealing. On the one hand he was keen—like many of his contemporaries—to have a *perfectly complete* 'generalised Euler formula' that covers everything. On the other hand he shrank from trivial complexities. So while he claimed that his formula was 'complete, all-embracing', he added confusedly that 'special cases can make the enumeration (of constituents) dubitable' (p. 103). That is, if an awkward polyhedron still defeats his formula, then its constituents were wrongly counted, and the monster should be adjusted by correct vision: *e.g.* the common vertices and edges of twintetrahedra should be seen and counted twice and each twin recognised as a separate polyhedron (*ibid.*). For further examples cf. p. 328, footnote 2.

[3] See Part III, pp. 229–234

ALPHA: I have a different point. We started from

(1) one vertex is one vertex.

We deduced from this

(2) $V = E$ for all perfect polygons.

We deduced from this

(3) $V - E + F = 1$ for all normal open polygonal systems.

From this

(4) $V - E + F = 2$ for all normal closed polygonal systems, i.e. polyhedra.

From this again in turn

(5) $V - E + F = 2 - 2(n - 1)$ for normal n-spheroid polyhedra.

(6) $V - E + F = 2 - 2(n - 1) + \sum_{k=1}^{F} e_k$ for normal n-spheroid polyhedra with multiply-connected faces.

(7) $V - E + F = \sum_{j=1}^{K} \left\{ 2 - 2(n - 1) + \sum_{k=1}^{F} e_k \right\}$ for normal n-spheroid polyhedra with multiply-connected faces and with cavities.

Isn't this a miraculous unfolding of the hidden riches of the trivial starting-point? And since (1) is indubitably true, so is the rest.

RHO [*aside*]: Hidden 'riches'? The last two only show how *cheap* generalisations may become![1]

LAMBDA: Do you really think that (1) is the single axiom from which all the rest follows? That deduction increases content?

ALPHA: Of course! Isn't this the miracle of the deductive thought-experiment? If once you have got hold of a little truth, deduction expands it infallibly into a tree of knowledge.[2] If a deduction does

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[1] Cf. pp. 328–9.

[2] Ancient philosophers did not hesitate to deduce a conjecture from a very trivial consequence of it (see, for example, our synthetic proof leading from the triangle to the polyhedron). Plato thought that 'a single axiom might suffice to generate a whole system'. 'Ordinarily he thought of a single hypothesis as fertile by itself, ignoring in his methodology the other premisses to which he is allying it' (Robinson [1953], p. 168). *This is characteristic of ancient informal logic, that is, of the logic of proof or of thoughtexperiment or of construction; we regard it as enthymematic only through hindsight: it was only later that an increase in content became a sign, not of the power, but of the weakness, of an inference.* This ancient informal logic was strongly advocated by Descartes, Kant and Poincaré; they all despised Aristotelian formal logic and dismissed it as sterile and irrelevant—at the same time extolling the infallibility of fertile informal logic.

not increase the content I would not call it deduction, but 'verification': 'verification differs from true demonstration precisely because it is purely analytic and because it is sterile.'[1]

LAMBDA: But surely deduction cannot increase content! If criticism reveals that the conclusion is richer than the premiss, we have to reinforce the premiss by making hidden lemmas explicit.

KAPPA: And it is these hidden lemmas that contain sophistication and fallibility and ultimately destroy the myth of infallible deduction.[2]

TEACHER: Any other question about Zeta's method?

(e) *Logical versus heuristic counterexamples.*

ALPHA: I like Zeta's *Rule 5*[3]—as I did Omega's *Rule 4*[4]. I liked Omega's method because it looked out for local but not global counterexamples: the ones which Lambda's original three rules[5] ignored as logically harmless, therefore heuristically uninteresting. Omega was stimulated by them to devise new thoughtexperiments: real advances in our knowledge.

Now Zeta is inspired by counterexamples that are both global and local—perfect corroborations from the logical but not from the heuristic point of view: although corroborations, they still call for action. Zeta proposes to extend, sophisticate our original thoughtexperiment, to turn logical corroborations into heuristic ones, logically satisfactory instances into instances that are satisfactory from both the logical and the heuristic point of view.

Both Omega and Zeta are for new ideas, while Lambda and especially Gamma are preoccupied with linguistic tricks to deal with their irrelevant global but not local counterexamples—the only relevant ones from their crankish point of view.

THETA: So the logical point of view is 'crankish', is it?

ALPHA: *Your* logical point of view, yes. But I want to make another remark. Whether deduction increases content or not—mind you, of course it does—it certainly seems to guarantee the *continuous*

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[1] Poincaré [1902], p. 33

[2] The hunt for hidden lemmas, which started only in mid-nineteenth century mathematical criticism, was closely related to the process that later replaced *proofs* by *proof-analyses* and *laws of thought* by *laws of language*. The most important developments in logical theory were usually preceded by the development of mathematical criticism. Unfortunately even the best historians of logic tend to pay exclusive attention to the *changes in logical theory* without noticing their roots in *changes in logical practice*. Cf. also p. 335, footnote 1.

[3] See p. 306

[4] See Part III, p. 237

[5] See Part III, p. 229

growth of knowledge. We start with a vertex and let knowledge grow forcefully and harmoniously to explain the relation between the number of vertices, edges and faces of any polyhedron whatsoever: an undramatic growth without refutations!

THETA [*to Kappa*]: Has Alpha lost all his judgment? One starts with a *problem*, not with a vertex![1]

ALPHA: This piecemeal but irresistibly victorious campaign will lead us to theorems that are 'not by themselves evident, but only deduced from true and known principles by the continuous and uninterrupted action of a mind that has a clear vision of each step in the process'.[2] They could never have been reached by 'unbiased' observation and a sudden flash of insight.

THETA: I am doubtful about this final victory. Such growth will never bring us to the cylinder—for (1) starts with a vertex and the cylinder has none. Also we may never reach onesided polyhedra, or many-dimensional polyhedra. This piecemeal continuous expansion may well stop at some point and you will have to look for a new, revolutionary start. And even this 'peaceful continuity' is full of refutations, criticism! Why do we go on from (4) to (5), from (5) to (6), from (6) to (7) if not under the continuous pressure of counterexamples which are both global and local? Lambda accepted as genuine counterexamples only those which are global but not local: they revealed the *falsehood* of the theorem. Omega's innovation—rightly praised by Alpha—was to regard also counterexamples which are local but not global as genuine counterexamples: they revealed the *poverty of the truth* of the theorem. Now Zeta tells us to recognise even those counterexamples as genuine which are both global and local: they too point to the *poverty of the truth* of the theorem. For example, picture-frames are both global and local counterexamples to Cauchy's theorem: they are of course corroborations as far as *truth* alone is concerned—but they are refutations as far as *content* is concerned. We may call the first (global but not local) counterexamples *logical*, the others *heuristic counterexamples.* But the more we recognise refutations—logical or heuristic—the quicker knowledge grows. Alpha regards logical counterexamples as irrelevant and refuses to call heuristic counterexamples counterexamples at all, because of his obsession with the idea that growth of mathematical knowledge is continuous, and criticism plays no role.

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[1] Alpha certainly seems to have slipped into the fallacy of deductive heuristic. Cf. p. 304, footnote 1.

[2] Descartes [1628], Rule III

ALPHA: You expand the concept of refutation and the concept of criticism artificially only to justify your critical theory of the growth of knowledge. Linguistic tricks as tools for a critical philosopher?

PI: I think a discussion of concept-formation may help us to elucidate the issue.

GAMMA: We are all ears.

8 Concept-formation

(a) Refutation by concept-stretching. A reappraisal of monsterbarring—and of the concepts of error and refutation

PI: I would first like to go back to the pre-Zeta, or even pre-Omega period, to the three main methods of theorem-formation: monsterbarring, exceptionbarring, and the method of proofs and refutations. Each started with the same naive conjecture, but ended up with different *theorems* and different *theoretical terms*. Alpha has already outlined some aspects of these differences,[1] but his account is unsatisfactory—especially in the case of monsterbarring and of the method of proofs and refutations. Alpha thought that the monsterbarring theorem 'hides behind the identity of the linguistic expression an essential improvement' on the naive conjecture: he thought that Delta gradually *contracted* the class of 'naive' polyhedra into a class purged of non-Eulerian monsters.

GAMMA: What is wrong with this account?

PI: That it was not the monsterbarrers who *contracted* concepts—it was the refutationists who *expanded* them.

DELTA: Hear, hear!

PI: Let us go back to the time of the first explorers of our subject. They were fascinated by the beautiful symmetry of *regular* polyhedra: they thought that the five regular bodies held the secret of the Cosmos.[2] By the time the Descartes-Euler conjecture was put forward, the concept of polyhedron included all sorts of convex polyhedra and even some concave polyhedra. But it certainly did not include polyhedra which were not simple, or polyhedra with ringshaped faces. For the polyhedra that they had in mind, the conjecture *was* true as it stood and the proof was flawless.[3]

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[1] See Part II, p. 138

[2] Cf. Lhuilier [1812-13*a*], p. 233

[3] Fig. 6 in Euler's [1750] is the first concave polyhedron ever to appear in a geometrical text. Legendre talks about convex and concave polyhedra in his [1794]. But before Lhuilier nobody mentioned concave polyhedra that were not simple.

However, one interesting qualification might be added. The first class of polyhedra ever investigated consisted partly of the five ordinary regular polyhedra and

Then came the refutationists. In their critical zeal they stretched the concept of polyhedron, to cover objects that were alien to the intended interpretation. The conjecture was true in its *intended interpretation*, it was only false in an *unintended interpretation* smuggled in by the refutationists. Their 'refutation' revealed no *error* in the original conjecture, no *mistake* in the original proof: it revealed the falsehood of a *new* conjecture which nobody had stated or thought of before.

Poor Delta! He valiantly defended the original interpretation of polyhedron. He countered each counterexample with a new clause to safeguard the original concept. . . .

GAMMA: But wasn't it Delta who shifted his position each time? Whenever we produced a new counterexample, he changed his definition for a longer one which displayed another of his 'hidden' clauses!

PI: What a monstrous appraisal of monsterbarring! He only *seemed* to shift his position. You wrongly accused him of using surreptitious terminological epicycles in the defence of a stubborn idea. His misfortune was that portentous *Definition 1*: 'A polyhedron is a solid whose surface consists of polygonal faces', which the refutationists seized upon immediately. But Legendre meant it to cover *only* his naive polyhedra; that it covered far more was entirely unrealised and unintended by its proposer. The mathematical public was willing to stomach the monstrous content which slowly emerged from this plausible, innocent-looking definition. This is why Delta had to stutter time and time again, 'I meant . . .', and had to keep making his endless 'tacit' clauses explicit: all because the naive concept had never been pinned down, and a simple, but monstrous, unintended definition had superseded it. But imagine a different

quasi-regular polyhedra like prisms and pyramids (cf. Euclid). This class was extended after the Renaissance in two directions. One is indicated in the text: to include all convex and some mildly indented simple polyhedra. The other was Kepler's: he widened the class of regular polyhedra by his invention of regular star-polyhedra. But Kepler's innovation was forgotten, only to be made again by Poinsot (cf. Part I, pp. 18–19). Euler surely did not dream of star-polyhedra. Cauchy knew of them, but his mind was strangely compartmentalised: when he had an interesting idea about star-polyhedra he published it; but he ignored star-polyhedra when presenting counterexamples to his general theorems about polyhedra. Not so the young Poinsot ([1809])—but later he changed his mind (cf. Part II, p. 128).

Thus Pi's statement, although heuristically correct (i.e. true in a rational history of mathematics), is historically false. (This should not worry us: actual history is frequently a caricature of its rational reconstructions.)

situation, where the definition fixed the intended interpretation of 'polyhedron' properly. Then it would have been up to the refutationists to devise ever longer *monster-including definitions* for say, 'complex polyhedra': 'A complex polyhedron is an aggregate of (real) polyhedra such that each two of them are soldered by congruent faces'. 'The faces of complex polyhedra can be complex polygons that are aggregates of (real) polygons such that each two of them are soldered by congruent edges'. This *complex polyhedron* would then correspond to Alpha's and Gamma's refutation-generated concept of *polyhedron*—the first definition allowing also for polyhedra that are not simple, the second also for faces that are not simply-connected. So devising new definitions is not necessarily the task of monsterbarrers or concept-preservers—it can also be that of monster-includers or concept-stretchers.[1]

SIGMA: Concepts and definitions—that is, intended concepts and unintended definitions—can then play funny tricks on each other! I never dreamt that concept-formation might lag behind an unintendedly wide definition!

PI: It might. Monsterbarrers only keep to the original concept, while concept-stretchers widen it; the curious thing is that concept-stretching goes on surreptitiously: nobody is aware of it, and since everybody's 'coordinate-system' expands with the widening concept, they fall prey to the heuristic delusion that monsterbarring *narrows* concepts, while in fact it keeps them invariant.

DELTA: Now who was intellectually dishonest? Who made surreptitious changes in his position?

GAMMA: I admit we were wrong in indicting Delta for surreptitious contractions of his concept of polyhedron: all his six definitions denoted the same good old concept of polyhedron he inherited from his forefathers. *He defined the very same poor concept in increasingly rich theoretical frames of reference, or languages: monsterbarring does not form concepts but only translates definitions.* The monsterbarring theorem is no improvement on the naive conjecture.

DELTA: Do you mean that all my definitions were logically equivalent?

GAMMA: That depends on your logical theory—according to mine they certainly are not.

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[1] An interesting example of monster-including definition is Poinsot's re-definition of *convexity*, which brings star-polyhedra into the respectable class of convex regular bodies [1809].

DELTA: This was not a very helpful answer, you will admit. But tell me, did you refute the naive conjecture? You refuted it only by surreptitiously perverting its original interpretation!

GAMMA: Well, we refuted it in a more imaginative and interesting interpretation than you ever dreamt of. This is what makes the difference between *refutations which only reveal a silly mistake* and *refutations which are major events in the growth of knowledge.* If you had found that 'for all polyhedra $V-E+F=1$' because of inept counting, and I had corrected you, I wouldn't call that a 'refutation'.

BETA: Gamma is right. After Pi's revelation we might hesitate to call our 'counterexamples' *logical counterexamples,* since they are after all not inconsistent with the conjecture in its intended interpretation; but they are certainly *heuristic counterexamples* since they spur the growth of knowledge. If we were to accept Delta's narrow logic, knowledge would not grow. Just suppose that somebody with the narrow conceptual framework discovers the Cauchy proof of the Euler conjecture. He finds that all the steps of this thoughtexperiment can easily be performed on *any* polyhedron. He takes the 'fact' that all polyhedra are simple and that all faces are simply-connected as obvious, as indubitable. It never occurs to him to turn his 'obvious' lemmas into conditions in an improved conjecture and so to build up a theorem—because the stimulus of counterexamples, in showing up some 'trivially true' lemmas as false, is missing. Thus he thinks that the 'proof' indubitably establishes the truth of the naive conjecture, that its certainty is beyond doubt. But his 'certainty' is far from being a sign of success, it is only a symptom of lack of imagination, of conceptual poverty. It produces smug satisfaction and prevents the growth of knowledge.[1]

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[1] This is in fact Cauchy's case. It is unlikely that if Cauchy had already discovered his revolutionary exception-barring method (cf. Part III, pp. 234-235), he would not have searched for and found some exceptions. But he probably came across the problem of exceptions only later, when he decided to clear up the chaos in analysis. (It was Lhuilier who seems to have first noticed, and faced, the fact that such 'chaos' was not confined to analysis.)

Historians, e.g. Steinitz in his [1914-31], usually say that Cauchy, noticing that his theorem was not universally valid, stated it for *convex* polyhedra only. It is true that in his proof he uses the expression 'the convex surface of a polyhedron' ([1811], p. 81), and in his [1812] he restates Euler's theorem under the general head: 'Theorems on solid angles and *convex polyhedra*'. But probably to counteract this title, he gives particular stress to the *universal* validity of Euler's theorem for *any* polyhedron (Theorem XI, p. 94), while stating three other theorems (Theorem XIII and its two

(*b*) *Proof-generated versus naive concepts. Theoretical versus naive classification*

Pi: Let me return to the proof-generated theorem: 'All simple *polyhedra* with simply-connected faces are Eulerian'. This formulation is misleading. It should read: 'All simple *objects* with simply-connected faces are Eulerian.'

Gamma: Why?

corollaries) explicitly for *convex* polyhedra (pp. 96 and 98).

Why Cauchy's sloppy terminology? Cauchy's concept of polyhedron *almost* coincided with the concept of convex polyhedron. But it did not coincide exactly: Cauchy knew about concave polyhedra, which can be obtained by slightly pushing in the side of convex polyhedra, but he did not discuss what seemed to be irrelevant further *corroborations*—not *refutations*—of his theorem. (*Corroborations never compare with counterexamples, or even 'exceptions', as catalysts for the growth of concepts.*) This is the reason for Cauchy's casual use of 'convex': it was a failure to realise that concave polyhedra might give counterexamples, not a conscious effort to *eliminate* these counterexamples. In the very same paragraph, he argues that Euler's theorem is an 'immediate consequence' of the lemma that $V-E+F=1$ for *flat* polygonal networks, and states that 'for the validity of the theorem $V-E+F=1$ it has no significance whatever whether the polygons lie in the same plane or in different planes, since the theorem is concerned only with the number of polygons and the number of their constituents' (p. 81). This argument is perfectly correct within Cauchy's narrow conceptual framework, but incorrect in a wider one, in which 'polyhedron' refers also to, say, picture-frames. The argument was frequently repeated in the first half of the nineteenth century (e.g. Olivier [1826], p. 230, or Grunert [1827], p. 367, or R. Baltzer [1860-62], Vol. II, p. 207). It was criticised by J. C. Becker ([1869], p. 68).

Often, as soon as concept-stretching refutes a proposition, the refuted proposition seems such an elementary mistake that one cannot imagine that great mathematicians could have made it. This important characteristic of concept-stretching refutation explains why respectful historians, because they do not understand that concepts *grow*, create for themselves a maze of problems. After saving Cauchy by claiming that he 'could not possibly miss' polyhedra which are not simple and that therefore he '*categorically*' (!) *restricted* the theorem to the domain of convex polyhedra, the respectful historian now has to explain why Cauchy's borderline was 'unnecessarily' narrow. Why did he *ignore* non-convex Eulerian polyhedra? Steinitz's explanation is this: the *correct* formulation of the Euler-formula is in terms of connectivity of surfaces. Since in Cauchy's period this concept was not yet 'clearly grasped', 'the simplest way out' was to assume convexity (p. 20). So Steinitz explains away a mistake that Cauchy never made.

Other historians proceed in a different way. They say that before the point where the correct conceptual framework (i.e. the one they know) was reached there was only a 'dark age' with 'seldom, if ever, sound' results. This point in the theory of polyhedra is Jordan's proof (1866) according to Lebesgue ([1923], pp. 59-60); it is Poincaré's (1895) according to Bell ([1945], p. 460).

PI: The first formulation suggests that the class of simple polyhedra that occurs in the theorem is a subclass of the class of ' polyhedra ' of the naive conjecture.

SIGMA: Of course the class of simple polyhedra is a subclass of polyhedra! The concept of ' simple polyhedron ' *contracts* the original wide class of polyhedra by restricting them to those on which the first lemma of our proof is performable. The concept of ' simple polyhedron with simply-connected faces ' indicates a further contraction of the original class. . . .

PI: No! The original class of polyhedra contained only polyhedra that were simple and whose faces were simply-connected. Omega was wrong when he said that lemma-incorporation reduces content.[1]

OMEGA: But doesn't each incorporation of lemmas rule out a counterexample?

PI: Of course it does: but a counterexample that was produced by concept-stretching.

OMEGA: So lemma-incorporation *conserves* content, just like monster-barring?

PI: No. Lemma-incorporation *increases* content: monster-barring does not.

OMEGA: What? Do you really want to convince me not only that lemma-incorporation does not *reduce* content, but also that it *increases* it? That instead of *contracting* concepts it *stretches* them?

PI: Exactly. Just listen. Was a globe, with a political map drawn on it, an element of the original class of polyhedra?

OMEGA: Certainly not.

PI: But it became one after Cauchy's proof. For you can perform Cauchy's proof on it without the slightest difficulty—if only there are no ringshaped countries or seas on it.[2]

GAMMA: That is right! Pumping the polyhedron up into a ball and distorting edges and faces will not perturb us in the least in performing the proof—so long as the distortion does not alter the *number* of vertices, edges and faces.

SIGMA: I see your point. Then the proof-generated ' simple polyhedron ' is not just a contraction, a specification, but also a *generalisation*, an *expansion* of the naive ' polyhedron '.[3] The idea of

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[1] See Part III, p. 236

[2] Cf. Part II, p. 132, footnote.

[3] Darboux, in his [1874], came close to this idea. Later it was clearly formulated by Poincaré: ' Mathematics is the art of giving the same name to different

generalising the concept of polyhedron so that it should include crumpled, *curvilinear* ' polyhedra ' with *curved* faces could hardly have occurred to anybody before Cauchy's proof; even if it had, it would have been dismissed as crankish. But now it is a natural generalisation, since the operations of our proof can be interpreted for them just as well as for ordinary naive polyhedra with straight edges and flat faces.[1]

PI: Good. But you have to make one more step. *Proof-generated concepts* are neither ' specifications ', nor ' generalisations ' of naive concepts. The impact of proofs and refutations on naive concepts is much more revolutionary than that: they *erase* the crucial naive concepts completely and *replace* them by proof-generated concepts.[2]

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things.... If one chooses the right language, one is surprised to learn that the proofs made for a known object apply immediately to many new objects, without the slightest change—one can even retain the names' ([1908], p. 375). Fréchet calls this ' an extremely useful principle of generalisation ', and formulates it as follows: ' When the set of properties of a mathematical entity used in the proof of a proposition about this entity does not determine this entity, the proposition can be extended to apply to a more general entity' ([1928], p. 18). He points out that such generalisations are not trivial and ' may require very great efforts ' (*ibid.*).

[1] Cauchy did not notice this. His proof differed from the one given by the Teacher in one important respect: Cauchy in his [1811-12] did *not* imagine the polyhedron to be made of rubber. The novelty of his proof-idea was to imagine the polyhedron as a *surface*, and not as a *solid*, as Euclid, Euler and Legendre did. But he imagined it as a *solid* surface. When he removed one face and mapped the remaining spatial polygonal network into a flat polygonal network, he did not conceive his mapping as a *stretching* that might *bend* faces or edges. The first mathematician to notice that Cauchy's proof could be performed on polyhedra with *bent faces* was Crelle ([1826-27], pp. 671-2), but he still carefully stuck to *straight edges*. For Cayley however it seemed recognisable ' *at first sight* ' that ' the theory would not be materially altered by allowing the edges to be curved lines ' ([1861], p. 425). The same remark was made independently in Germany by Listing ([1861], p. 99) and in France by Jordan ([1866], p. 39).

[2] *This theory of concept-formation weds concept-formation to proofs and refutations.* Pólya weds it to *observations*: ' When the physicists started to talk about " electricity," or the physicians about " contagion," these terms were vague, obscure, muddled. The terms that the scientists use today, such as " electric charge," " electric current," " fungus infection," " virus infection," are incomparably clearer and more definite. Yet what a tremendous amount of observation, how many ingenious experiments lie between the two terminologies, and some great discoveries too. Induction changed the terminology, clarified the concepts. We can illustrate also this aspect of the process, the inductive clarification of concepts, by suitable mathematical examples ' ([1954], Vol. I, p. 55). But even this mistaken inductivist theory of concept-formation is preferable to the attempt to make concept-formation *autonomous*, to make ' *clarification* ' or ' *explication* ' of concepts a *preliminary* to any scientific discussion.

The naive term ' polyhedron ', even after being stretched by refutationists, denoted something that was crystal-like, a solid with ' plane ' faces, straight edges. The proof-ideas swallowed this naive concept and fully digested it. In the different proof-generated theorems we have nothing of the naive concept. That disappeared without trace. Instead each proof yields its characteristic proof-generated concepts, which refer to stretchability, pumpability, photographability, projectability and the like. The old problem disappeared, new ones emerged. After Columbus one should not be surprised if *one does not solve the problem one has set out to solve.*

SIGMA: So the ' theory of solids ', the original ' naive ' realm of the Euler conjecture, dissolves, and the remodelled conjecture reappears in projective geometry if proved by Gergonne, in analytical topology if proved by Cauchy, in algebraic topology if proved by Poincaré. . . .

PI: Quite right. And now you will understand why I formulated the theorems not, like Alpha or Beta, as: ' All Gergonne-polyhedra are Eulerian ', ' All Cauchy-polyhedra are Eulerian ', and so on, but rather as: ' All Gergonnian objects are Eulerian ', ' All Cauchy objects are Eulerian ', and so on.[1] *So I find it uninteresting to quarrel not only about the exactness of naive concepts but also about the truth or falsehood of naive conjectures.*

BETA: But surely we can retain the term ' polyhedron ' for our favourite proof-generated term, say, ' Cauchy-objects '?

PI: If you like, but remember that *your term no longer denotes what it set out to denote*: that its naive meaning has disappeared and that now it is used. . . .

BETA: . . . for a more general, improved concept!

THETA: No! For a totally different, novel concept.

SIGMA: I think your views are paradoxical!

PI: If you mean by paradoxical ' an opinion not yet generally received ',[2] and possibly inconsistent with some of your ingrained naive ideas, never mind: you only have to replace your naive ideas with the paradoxical ones. This may be a way to ' solve ' paradoxes. But what particular view of mine do you have in mind?

SIGMA: You remember, we found that some star-polyhedra are Eulerian while some others are not. We were looking for a proof that would be deep enough to explain the Eulerianness both of ordinary and star-polyhedra. . . .

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[1] See Part III, p. 245

[2] Hobbes [1656], Animadversions upon the Bishop's Reply No. xxi.

EPSILON: I have it.[1]

SIGMA: I know. But just for the sake of argument let us imagine that there is no such proof, but that somebody offers, in addition to Cauchy's proof for Eulerian 'ordinary' polyhedra, a corresponding but altogether different proof for Eulerian star-polyhedra. Would you then, Pi, because of these two different proofs, propose to split into two what we formerly classified as one? And would you have two completely different things united under one name just because somebody finds a common explanation for some of their properties?

PI: Of course I would. I certainly wouldn't call a whale a fish, a radio a noisy box (as aborigines may do), and I am not upset when a physicist refers to glass as a liquid. Progress indeed replaces *naive classification* by *theoretical classification*, that is, by theory-generated (proof-generated, or if you like, explanation-generated) classification. Conjectures and concepts both have to pass through the purgatory of proofs and refutations. *Naive conjectures and naive concepts are superseded by improved conjectures (theorems) and concepts (proof-generated or theoretical concepts) growing out of the method of proofs and refutations.* And as theoretical ideas and concepts supersede naive ideas and concepts, theoretical language supersedes naive language.[2]

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[1] See Part III, p. 244, footnote 1.

[2] It is interesting to follow the gradual changes from the rather naive classification of polyhedra to the highly theoretical one. The first naive classification which covers not only simple polyhedra comes from Lhuilier: a classification according to the number of *cavities, tunnels* and '*inner polygons*' (see p. 310, footnote 1).

(*a*) *Cavities.* Euler's first proof and, incidentally, Lhuilier's own ([1812–13], pp. 174–177), rested on the decomposition of the *solid*, either by cutting off its corners one by one, or by decomposing it into pyramids from one or more points in the inside. Cauchy's proof-idea however—Lhuilier did not know about it—rested on the decomposition of the polyhedral *surface*. When the theory of polyhedral surfaces finally superseded the theory of polyhedral solids, cavities became uninteresting: *one* 'polyhedron with cavities' turns into a whole *class* of polyhedra. Thus our old monster-barring *Definition 2* (Part I, p. 16) became a proof-generated, theoretical definition, and the taxonomical concept of 'cavity' disappeared from the mainstream of growth.

(*b*) *Tunnels.* Already Listing pointed to the unsatisfactoriness of this concept (see p. 310, footnote 1). The replacement came not from any 'explication' of the 'vague' concept of tunnel, as a Carnapian might be tempted to expect, but from trying to prove and refute Lhuilier's naive conjecture about the Euler-characteristic of polyhedra with tunnels. In the course of this process the concept of polyhedron with *n* tunnels disappeared and proof-generated 'multiply-connectedness' (what we called '*n*-spheroidness') took its place. In some papers we find the naive term retained for the new proof-generated concept: Hoppe defines the number of 'tunnels'

OMEGA: In the end we shall arrive from naive, accidental, merely nominal classification to the final true, real, classification, to perfect language![1]

(c) *Logical and heuristic refutations revisited*

PI: Let me take up again some of the issues which have arisen in connection with deductive guessing. First let us take the problem of heuristic versus logical counterexamples as raised in the discussion between Alpha and Theta.

My exposition has shown, I think, that even the so-called 'logical' counterexamples were heuristic. In the originally intended interpretation there is no inconsistency between

(*a*) All polyhedra are Eulerian

and

(*b*) The pictureframe is not Eulerian.

If we keep to the tacit semantical rules of our original language our counterexamples are not counterexamples. They are turned into logical counterexamples only by changing the rules of the language by concept-stretching.

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by the number of cuts that leave the polyhedron connected ([1879], p. 102). For Ernst Steinitz the concept of tunnel is already so theory-impregnated that he is unable to find any 'essential' difference between Lhuilier's naive classification according to the number of tunnels and the proof-generated classification according to multiply-connectedness; therefore he regards Listing's criticism of Lhuilier's classification as 'largely unjustified' ([1914-31], p. 22).

(*c*) '*Inner polygons*'. This naive concept too was soon replaced, first by ring-shaped, then by multiply-connected, faces (also cf. p. 310, footnote 1), (*replaced*, not 'explicated', for 'ring-shaped face' is surely not an explication of 'inner polygon'). When, however, the theory of polyhedral surfaces was superseded on the one hand by the topological theory of surfaces, and on the other hand by graph-theory, the problem of how multiply-connected faces influence the Euler-characteristic of a polyhedron lost all its interest.

Thus, out of the three key concepts of the first naive classification, only one was left', and even that in a hardly recognisable form—the generalised Euler formula was, for the moment, reduced to $V-E+F=2-2n$. (For further developments cf. p. 329, footnote 1.)

[1] As far as naive classification is concerned, nominalists are close to the truth when claiming that the only thing that polyhedra (or, to use Wittgenstein's favourite example, games) have in common is their name. But after a few centuries of proofs and refutations, as the theory of polyhedra (or, say, the theory of games) develops, and theoretical classification replaces naive classification, the balance changes in favour of the realist. The problem of universals ought to be reconsidered in view of the fact that, as knowledge grows, languages change.

GAMMA: Do you mean that *all* interesting refutations are heuristic?

PI: Exactly. You cannot separate refutations and proofs on the one hand and changes in the conceptual, taxonomical, linguistic framework on the other. Usually, when a 'counterexample' is presented, you have a choice: either you refuse to bother with it, since it is not a counterexample at all in your *given* language L_1, or you agree to change your language by concept-stretching and accept the counterexample in your new language L_2. . . .

ZETA: . . . and *explain* it in L_3!

PI: According to traditional static rationality you would have to make the first choice. Science teaches you to make the second.

GAMMA: That is, we may have two statements that are consistent in L_1, but we switch to L_2 in which they are inconsistent. Or, we may have two statements that are inconsistent in L_1, but we switch to L_2 in which they are consistent. As knowledge grows, languages change. 'Every period of creation is at the same time a period in which the language changes.'[1] The growth of knowledge cannot be modelled in any given language.

PI: That is right. Heuristic is concerned with language-dynamics, while logic is concerned with language-statics.

(*d*) *Theoretical versus naive concept-stretching. Continuous versus critical growth*

GAMMA: You promised to come back to the question whether or not deductive guessing offers us a continuous pattern of the growth of knowledge.

PI: Let me first sketch some of the many *historical* forms which this *heuristic* pattern can take.

The *first main pattern* is when naive concept-stretching outstrips theory by far and produces a vast chaos of counterexamples: our naive concepts are loosened but no theoretical concepts replace them. In this case deductive guessing may catch up—piecemeal—with the backlog of counterexamples. This is, if you like, a continuous 'generalising' pattern—but do not forget that it starts with refutations, that its continuity is the piecemeal explanation by a growing theory of the heuristic refutations of its first version.

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[1] Félix [1957], p. 10. According to logical positivists, the *exclusive* task of philosophy is to construct 'formalised' languages in which artificially congealed states of science are expressed (see our quotation from Carnap in Part I, p. 2). But such investigations scarcely get under way before the rapid growth of science discards the old 'language system'. Science teaches us not to respect any given conceptual-linguistic framework lest it should turn into a conceptual prison—language analysts have a vested interest in at least slowing down this process, in order to justify their linguistic therapeutics, that is, to show that they have an all-important feedback to, and value for, science, that they are not degenerating into 'fairly dried-up pettyfoggery' (Einstein [1953]). Similar criticisms of logical positivism have been made by Popper: see e.g. his [1934], p. 128, footnote *3.

GAMMA: Or, 'continuous' growth only indicates that refutations are miles ahead!

PI: That is right. But it may happen that each single refutation or expansion of naive concepts is *immediately* followed by an expansion of the theory (and theoretical concepts) which explains the counterexample; 'continuity' then gives place to an exciting alternation of concept-stretching refutations and ever more powerful theories, of *naive concept-stretching* and explanatory *theoretical concept-stretching*.

SIGMA: Two accidental historical variations on the same heuristic theme!

PI: Well, there is not really much difference between them. In both of them *the power of the theory lies in its capacity to explain its refutations in the course of its growth*. But there is a *second main pattern* of deductive guessing. . . .

SIGMA: Yet another accidental variation?

PI: Yes, if you like. In this variation however the growing theory not only *explains* but *produces* its refutations.

SIGMA: What?

PI: In this case theoretical growth overtakes—and, indeed, eliminates—naive concept-stretching. For example, one starts with, say, Cauchy's theorem, without a single counterexample on the horizon. Then one tests the theorem by transforming the polyhedron in all possible ways: cutting it into two, cutting off pyramidal corners, bending it, distorting it, pumping it up. . . . Some of these test-ideas will lead to proof-ideas [1] (by arriving at something known to be true and then turning back, that is, by following the Pappian analysis-synthesis pattern), but some—like Zeta's 'double-pasting test'—will lead us, not back to something already known, but to real novelty, to some heuristic refutation of the tested proposition—*not by extending a naive concept, but by extending the theoretical framework*. This sort of refutation is self-explanatory. . . .

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[1] Pólya discriminates between 'simple' and 'severe' tests. 'Severe' tests may give 'the first hint of a proof' ([1954], Vol. I, pp. 34-40).

IOTA: How dialectical! Tests turning into proofs, counterexamples that become examples by the very method of their construction. . . .

PI: Why dialectical? The test of one proposition turns into the proof of *another*, deeper proposition, counterexamples of the first into examples of the second. Why call confusion dialectic? But let me come back to my point: I do not think that my second main pattern of deductive guessing could be regarded—as Alpha would have it—as continuous growth of knowledge.

ALPHA: Of course it can. Compare our method with Omega's idea of replacing one proof-idea with a radically different, deeper one. Both methods increase content, but while in Omega's method one *replaces* operations of the proof that are applicable in a narrow domain by operations which are applicable in a wider domain, or, more radically, replaces the whole proof by one that is applicable in a wider domain—deductive guessing *extends* the given proof by adding operations which widen its applicability. Is this not continuity?

SIGMA: That is right! We deduce from the theorem a chain of ever wider theorems! From the special case ever more general cases! Generalisation by deduction![1]

PI: But full of counterexamples, once you recognise that *any* increase of content, *any* deeper proof follows or generates heuristic refutations of the previous poorer theorems. . . .

ALPHA: Theta expanded 'counterexample' to cover heuristic counterexamples. You now expand it to cover heuristic counterexamples that never actually exist. Your claim that your 'second pattern' is full of counterexamples is based on the expansion of the concept of counterexample to counterexamples with zero life-time, whose discovery coincides with their explanation! But why should all intellectual activity, every struggle for increased content in a unified theoretical framework, be 'critical'? Your dogmatic 'critical attitude' is obscuring the issue!

TEACHER: The issue between you and Pi is certainly obscure—for your 'continuous growth' and Pi's 'critical growth' are perfectly consistent. I am more interested in the *limitations*, if any, of deductive guessing, or 'continuous criticism'.

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[1] In *informal* logic there is nothing wrong with the 'fact, so usual in mathematics and still so surprising to the beginner, or to the philosopher who takes himself for advanced, that the general case can be logically equivalent to a special case' (Pólya [1954], Vol. I, p. 17). Also cf. Poincaré [1902], pp. 31-33.

(e) *The limits of the increase in content. Theoretical versus naive refutations*

PI: I think that sooner or later 'continuous' growth is bound to reach a dead-end, a *saturation point* of the theory.

GAMMA: But surely I can always stretch some of the concepts!

PI: Of course. *Naive* concept-stretching may go on—but *theoretical* concept-stretching has limits! Refutations by naive concept-stretching are only gadflies that prod us to catch up by theoretical concept-stretching. So there are two sorts of refutations. We *stumble* on the first sort by coincidence or good fortune, or by an arbitrary expansion of some concept. They are like miracles, their 'anomalous' behaviour is unexplained; we accept them as *bona fide* counterexamples only because we are used to accepting concept-stretching criticism. I shall call these *naive* counterexamples or *freaks.* Then there are the *theoretical counterexamples*: these are either originally produced by proof-stretching or, alternatively, they are freaks which are reached by stretched proofs, explained by them, and thereby raised to the status of theoretical counterexamples. Freaks have to be looked upon with great suspicion: they may not be genuine counter-examples, but instances of a quite different theory—if not outright mistakes.

SIGMA: But what shall we do when we get stuck? When we cannot turn our naive counterexamples into theoretical ones by expanding our original proof?

PI: We may probe again and again whether or not our theory still has some hidden capacity for growth. Sometimes, however, we have good reason to give up. For instance, as Theta rightly pointed out, if our deductive guessing starts from a vertex we cannot very well ever expect it to explain the vertexless cylinder.

ALPHA: So after all, the cylinder was not a monster, but a freak!

THETA: But freaks should not be played down! They are the *real* refutations: they cannot be fitted into a pattern of continuous 'generalisations', and may actually force us to revolutionise our theoretical framework. . . .[1]

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[1] Cayley [1861] and Listing [1861] took the stretching of the basic concepts of the theory of polyhedra seriously. Cayley defined *edge* as 'the path from a summit to itself, or to any other summit' but allowed edges to degenerate into vertexless closed curves, which he called 'contours' (p. 426). Listing had one term for edges, whether with two, one, or no vertices: '*lines*' (p. 104). Both realised that a completely new theory was needed to explain the 'freaks' which they naturalised with their liberal conceptual framework—Cayley invented the '*Theory of Partitions of a Close*', Listing, one of the great pioneers of modern topology, the '*Census of Spatial Complexes*'.

OMEGA: Good! One may get to a *relative saturation point* of a *particular* chain of deductive guessing—but then one finds a revolutionary, new, deeper proof-idea that has more explanatory power. At the end one still gets to a *final* proof—without limit, without saturation point, without freaks to refute it!

PI: What? A single unified theory to explain *all* the phenomena of the universe? Never! Sooner or later we shall approach something like an *absolute saturation point.*

GAMMA: I don't really mind whether we do or not. If a counterexample can be explained by a cheap, *trivial* extension of the proof, I would already regard it as a freak. I repeat: I really do not see any point in generalising 'polyhedron' to include a polyhedron with cavities: this is not one polyhedron, but a class of polyhedra. I would also forget about 'multiply-connected faces'—why not draw the missing diagonals? As to the generalisation that includes twin-tetrahedra, I would reach for my gun: it only serves for making up complicated, pretentious formulas for nothing.

RHO: At last you rediscover my method of monster-adjustment![1] It relieves you of shallow generalisation. Omega should not have called content 'depth'; *not every increase in content is also an increase in depth:* think of (6) and (7)![2]

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[1] See Part II, pp. 127-130 and pp. 135-136

[2] Quite a few mathematicians cannot distinguish the trivial from the non-trivial. This is especially awkward when a lack of feeling for relevance is coupled with the illusion that one can construct a *perfectly complete* formula that covers all conceivable cases (cf. p. 310, footnote 2). Such mathematicians may work for years on the 'ultimate' generalisation of a formula, and end up by extending it with a few trivial corrections. The excellent mathematician, J. C. Becker, provides an amusing example: after many years' work he produced the formula $V-E+F=4-2n+q$ where n is the number of cuts that is needed to divide the polyhedral surface into simply-connected surfaces for which $V-E+F=1$, and q is the number of diagonals that one has to add to reduce all the faces to simply-connected ones ([1869], p. 72). He was very proud of his achievement, which—he claimed—shed 'completely new light', and even 'brought to a conclusion' 'a subject in which people like Descartes, Euler, Cauchy, Gergonne, Legendre, Grunert, and von Staudt, took interest' before him (p. 65). But three names were missing from his reading list: Lhuilier, Jordan and Listing. When he was told about Lhuilier, he published a sad note, admitting that Lhuilier knew all this more than fifty years before. As for Jordan, he was not interested in ring-shaped faces, but happened to take an interest in open polyhedra with boundaries, so that in his formula m, the number of boundaries, figures in addition to n ([1866*a*], p. 86). So Becker—in a new paper [1869*a*]—combined Lhuilier's and Jordan's formulas into $V-E+F=2-2n+q+m$ (p. 343). But in his embarrassment he was too hasty, and had not digested Listing's long paper. So he sadly

ALPHA: So you would stop at (5) in my series?

GAMMA: Yes. (6) and (7) are not growth, but degeneration! Instead of going on to (6) and (7), I would rather find and explain some *exciting* new counterexample! [1]

ALPHA: You may be right after all. But who decides *where* to stop? Depth is only a matter of taste.

GAMMA: Why not have mathematical critics just as you have literary critics, to develop mathematical taste by public criticism? We may even stem the tide of pretentious trivialities in mathematical literature.[2]

SIGMA: If you stop at (5) and turn the theory of polyhedra into a theory of triangulated spheres with *n* handles, how can you, if the need arises, deal with trivial anomalies like those explained in (6) and (7)?

MU: Child's play!

THETA: Right. Then we stop at (5) for the moment. *But can we stop*? Concept-stretching may refute (5)! We may ignore the stretching of a concept if it yields a counterexample that shows up the poverty of the content of our theorem. But if the stretching yields a counterexample that shows up its plain falsehood, what then? We may refuse to apply our content-increasing *Rule 4* or *Rule 5* to explain a freak, but we have to apply our content-preserving *Rule 2* to ward off refutation by a freak.

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concluded his [1869*a*] with 'Listing's generalisation is still wider'. (By the way, he later tried to extend his formula also to star-polyhedra ([1874]; cf. Part II, p. 128, footnote 2.)

[1] Some people may entertain philistine ideas about *a law of diminishing returns in refutations*. Gamma, for one, certainly does not. We shall not discuss *one-sided* polyhedra (Möbius, [1865]) or *n-dimensional* polyhedra (Schläfli, [1852]). These would confirm Gamma's expectation that totally unexpected concept-stretching refutations may always give the whole theory a new—possibly revolutionary—push.

[2] Pólya points out that shallow, cheap, generalisation is 'more fashionable nowadays than it was formerly. It dilutes a little idea with a big terminology. The author usually prefers to take even that little idea from somebody else, refrains from adding any original observation, and avoids solving any problem except a few problems arising from the difficulties of his own terminology. It would be very easy to quote examples, but I don't want to antagonize people' ([1954], Vol. I, p. 30). Another of the greatest mathematicians of our century, John von Neumann, also warned against this 'danger of degeneration', but thought it would not be so bad 'if the discipline is under the influence of men with an exceptionally well-developed taste' ([1947], p. 196). One wonders, though, whether the 'influence of men with an exceptionally well-developed taste' will be enough to save mathematics in our 'publish or perish' age.

GAMMA: That is it! We may dismiss cheap '*generalisations*', but we can hardly dismiss 'cheap' *refutations.*

SIGMA: Why not build up a monster-barring definition of 'polyhedron', adding a new clause for each freak?

THETA: In both cases our old nightmare, vicious infinity, is back again.

ALPHA: While you are increasing content, you develop ideas, do mathematics; after it you clarify concepts, do linguistics. Why not stop altogether when one stops increasing content? Why be trapped in vicious infinities?

MU: Not mathematics versus linguistics again! Knowledge never profits from such disputes.

GAMMA: The term 'never' soon turns into 'soon'. I am all for taking up our old discussion again.

MU: But we already ended up in a deadlock! Or does anybody have anything new to say?

KAPPA: I think I have.

9 *How Criticism may turn Mathematical Truth into Logical Truth*

(*a*) *Unlimited concept-stretching destroys meaning and truth*

KAPPA: Alpha already said that our 'old method' leads to vicious infinity.[1] Gamma and Lambda answered with the hope that the stream of refutations might peter out:[2] but now that we understand the mechanism of refutational success—concept-stretching—we know that theirs was a vain hope. For any proposition there is always some sufficiently narrow interpretation of its terms, such that it turns out true, and some sufficiently wide interpretation such that it turns out false. Which interpretation is intended and which unintended depends of course on our intentions. The first interpretation may be called the *dogmatist, verificationist or justificationist interpretation*, the second the *sceptical, critical or refutationist interpretation.* Alpha called the first a conventionalist stratagem[3]—but now we see that the second is one too. You all ridiculed Delta's dogmatist interpretations of the naive conjecture[4] and then Alpha's dogmatist interpretation of the theorem.[5] But concept-stretching will refute *any* statement, and will leave no true statement whatsoever.

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[1] See Part III, p. 232

[2] See Part III, p. 233

[3] Alpha in fact did not use this Popperian term explicitly; see Part I, p. 23.

[4] See Part I §4,(*b*)

[5] See Part III, §5

GAMMA: Wait. True, we stretched 'polyhedron'—then tore it up and threw it away: as Pi pointed out, the naive concept 'polyhedron' does not figure in the theorem any more.

KAPPA: But then you will start stretching a term in the theorem—a theoretical term, won't you? You yourself chose to stretch 'simply-connected face' to include the circle and the jacket of the cylinder.[1] You implied that it was a matter of intellectual honesty to stick one's neck out, to achieve the respectable status of refutability, i.e. to make the refutationist interpretation possible. But because of concept-stretching, refutability means refutation. So you slide onto the infinite slope, refuting *each* theorem and replacing it by a more 'rigorous' one—by one whose falsehood has not been 'exposed' yet! But *you never get out of falsehood.*

SIGMA: What if we stop at a certain point, adopt justificationist interpretations, and don't budge either from the truth or from the particular linguistic form in which that truth was expressed?

KAPPA: Then you will have to ward off concept-stretching counter-examples with monster-barring definitions. Thus you will slide on to another infinite slope: you will be forced to admit of each 'particular linguistic form' of your true theorem that it was not precise enough, and you will be forced to incorporate in it more and more 'rigorous' definitions couched in terms whose vagueness has not been exposed yet! But *you never get out of vagueness.*

THETA [*aside*]: What is wrong with a heuristic where vagueness is the price we pay for growth?

ALPHA: I told you: precise concepts and unshakable truths do not dwell in language, but only in thought!

GAMMA: Let me challenge you, Kappa. Take the theorem as it stood, after we took account of the cylinder: 'For all simple objects with simply-connected faces such that the edges of the faces terminate in vertices $V-E+F=2$.' How would you refute *this* by the method of concept-stretching?

KAPPA: First I go back to the defining terms and spell out the proposition in full. Then I decide which concept to stretch. For instance, 'simple' stands for 'stretchable onto a plane after having had a face removed'. I shall stretch 'stretching'. Take the already discussed twin-tetrahedra—the pair with an *edge* in common (Fig. 6*a*). It is simple, its faces are simply-connected, but $V-E+F=3$. So our theorem is false.

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[1] See Part III, pp. 221-225

GAMMA: But this twin-tetrahedron is *not* simple!

KAPPA: Of course it is simple. Removing any face, I can stretch it on to a plane. I just have to be careful, when I get to the critical edge, that I do not tear anything there when opening the second tetrahedron along that edge.

GAMMA: But this is not stretching! You *tear*—or *split*—the edge into two edges! You certainly cannot map one point onto two: *stretching is a bicontinuous one-one mapping*!

KAPPA: *Def. 9*? I am afraid this narrow, dogmatist interpretation of ' stretching ' does not appeal to *my* common sense. For instance,

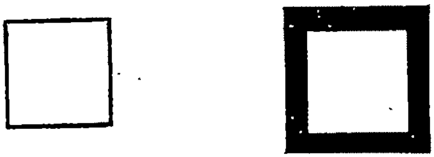
(a) (b)

FIG. 24

I can well imagine stretching a square (Fig. 24*a*) into two nested squares by stretching the boundary lines (Fig. 24*b*). Would you call this stretch a tear or a split, just because it is not a ' bicontinuous one-one mapping '? By the way, I wonder why you did not define stretching as a transformation that leaves *V*, *E* and *F* unaltered, and have done with it?

GAMMA: Right, you win again. I either have to agree to your refutationist interpretation of ' stretching ' and expand my proof, or find a deeper one, or incorporate a lemma—or I have to introduce a new monsterbarring definition. Yet in any of these cases I shall always make my defining terms clearer and clearer. Why should I not arrive at a point where the meanings of the terms will be so crystal clear that there will only be one single interpretation, as is the case with $2+2=4$? There is nothing elastic about the meaning of these terms and nothing refutable about the truth of this proposition, which shines for ever in the natural light of reason.

KAPPA: Dim light!

GAMMA: Stretch, if you can.

KAPPA: But this is child's play! In certain cases two and two make five. Suppose we ask for the delivery of two articles each weighing two pounds; they are delivered in a box weighing one pound; then in this package two pounds and two pounds will make five pounds!

GAMMA: But you get five pounds by adding *three* weights, 2 and 2 and 1!

KAPPA: True, our operation '2 and 2 make 5' is not an addition in the originally intended sense. But we can make the result hold true by a simple stretching of the meaning of addition. Naive addition is a very special case of packing where the weight of the covering material is zero. We have to build this lemma into the conjecture as a condition: our improved conjecture will be: '$2+2=4$ for "weightless" addition'.[1] The whole story of algebra is a series of such concept- and proof-stretchings.

GAMMA: I think you take 'stretching' a bit far. Next time you will interpret 'plus' as 'times' and consider it a refutation! Or you will interpret 'all' as 'no' in 'All polyhedra are polyhedra'! You stretch the concept of concept-stretching! We have to demarcate refutation by *rational stretching* from 'refutation' by *irrational stretching*. We cannot allow you to stretch any term you like just as you like.

We must pin down the concept of counterexample in crystal-clear terms!

DELTA: Even Gamma has turned into a monsterbarrer: now he wants a monsterbarring definition of concept-stretching refutation. *Rationality, after all, depends on inelastic, exact, concepts*![2]

KAPPA: *But there are no such concepts! Why not accept that our ability to specify what we mean is nil, therefore our ability to prove is nil?* If you want mathematics to be meaningful, you must resign of certainty. If you want certainty, get rid of meaning. You cannot have both. *Gibberish is safe from refutations, meaningful propositions are refutable by concept-stretching.*

GAMMA: Then your last statements can also be refuted—and you know it. 'Sceptics are not a sect of people who are persuaded of what they say, but a sect of liars.'[3]

KAPPA: Swear-words: the last resort of reason!

(b) *Mitigated concept-stretching may turn mathematical truth into logical truth*

THETA: I think Gamma is right about the need for demarcating rational from irrational concept-stretching. For concept-stretching has come a long way, and has changed from a mild, rational activity to a radical, irrational one.

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[1] Cf. Félix [1957], p. 9

[2] Gamma's demand for a crystal-clear definition of 'counterexample' amounts to a demand for crystal-clear, inelastic concepts in the metalanguage as a condition of rational discussion.

[3] Arnauld [1724], pp. xx-xxi

Originally, criticism concentrates exclusively on the *slight* stretching of *one particular* concept. It has to be *slight*, so that we do not notice it; if its real—stretching—nature were discovered, it might not be accepted as legitimate criticism. It concentrates on *one particular* concept, as in the case of our rather unsophisticated universal propositions: '*All A's are B's*'. Criticism then means finding a slightly stretched *A* (in our case *polyhedron*) that is not *B* (in our case *Eulerian*).

But Kappa sharpened this in two directions. First, to submit *more than one* constituent of the proposition under attack to concept-stretching criticism. Second, to turn concept-stretching from a surreptitious and rather modest activity into *open deformation* of the concept, like the deformation of 'all' into 'no'. Here any meaningful translation of the terms under attack that renders the theorem false is accepted as refutation. I would then say that *if a proposition cannot be refuted with respect to the constituents a, b, . . ., then it is logically true with respect to these constituents.*[1] Such a proposition is the end-result of a long critical-speculative process in the course of which the meaning-load of some terms is completely transferred to the remaining terms and to the form of the theorem.

Now all that Kappa says is that there are no propositions which are logically true with respect to *all* their constituents. But there may be logically true propositions with respect to *some* constituents, so that the stream of refutations can only be opened up again if new stretchable constituents are added. If we go the whole hog, we end up in irrationalism—but we need not. Now where should we draw the borderline? We may very well allow concept-stretching only for a distinguished subset of constituents which become the prime targets of criticism. Logical truth will not depend on their meaning.

SIGMA: So after all we took Kappa's point: we made truth independent of the meaning of at least *some* of the terms!

THETA: That is right. But if we want to defeat Kappa's scepticism, and escape his vicious infinities, we certainly have to stop concept-stretching at the point where it ceases to be a tool of growth and becomes a tool of destruction: we may have to find out which are those terms whose meaning can be stretched only at the cost of destroying the basic principles of rationality.[1]

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[1] This is a slightly paraphrased version of Bolzano's definition of logical truth ([1837], § 147). Why Bolzano, in the 1830's, proposed his definition, is a puzzling question, especially since his work anticipates the concept of model, one of the greatest innovations in nineteenth-century mathematical philosophy.

KAPPA: Can we stretch the concepts in your theory of critical rationality? Or will that be manifestly true, formulated in unstretchable, exact terms which do not need to be defined? Will your theory of criticism end in a 'retreat to commitment': is everything criticisable except for your theory of criticism, your 'metatheory'?[2]

OMEGA [*to Epsilon*]: I do not like this shift from Truth to rationality. *Whose* rationality? I sense conventionalist infiltration.

BETA: What are you talking about? I understand Theta's 'mild pattern' of concept-stretching. I also understand that concept-stretching may attack more than one term: we saw this when Kappa stretched 'stretching' or when Gamma stretched 'all'. . . .

SIGMA: Surely Gamma stretched 'simply-connected'!

BETA: But no. 'Simply-connected' is an abbreviation—he only stretched the term 'all' that occurred among the defining terms.[3]

THETA: Come back to the point. You are unhappy about 'open', radical concept-stretching?

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[1] Nineteenth-century mathematical criticism stretched more and more concepts, and shifted the meaning-load of more and more terms onto the *logical form* of the propositions and onto the meaning of the few (as yet) unstretched terms. In the 1930's this process seemed to slow down and the demarcation line between unstretchable ('logical') terms and stretchable ('descriptive') terms seemed to become stable. A list, containing a small number of logical terms came to be widely agreed upon, so that a general definition of logical truth became possible; logical truth was no longer 'with respect to' an *ad hoc* list of constituents. (Cf. Tarski [1935].) Tarski was however puzzled about this demarcation and wondered whether, after all, he would have to return to a relativised concept of counterexample, and consequently, of logical truth (p. 420)—like Bolzano's, of which, by the way, Tarski did not know. The most interesting result in this direction was Popper's [1947-48] from which it follows that one cannot give up further logical constants without giving up some basic principles of rational discussion.

[2] 'Retreat to commitment' is Bartley's expression [1962]. He investigates the problem of whether a rational defence of critical rationalism is possible mainly with respect to *religious* knowledge—but the problem-patterns are very much the same with respect to *mathematical* knowledge.

[3] See Part III, pp. 221-225. Gamma did, in fact, want to remove some meaning-load from 'all', so that it no longer applied only to non-empty classes. The modest stretching of 'all' by removing 'existential import' from its meaning and thereby turning the empty set from a monster into an ordinary *bourgeois* set was an important event—connected not only with the Boolean set-theoretical re-interpretation of Aristotelian logic, but also with the emergence of the concept of vacuous satisfaction in mathematical discussion.

Beta: Yes. Nobody would accept this last brand as genuine refutation! I quite see that the mild concept-stretching trend of heuristic criticism that Pi uncovered is a most important vehicle of mathematical growth. But mathematicians will never accept this last, wild form of refutation!

Teacher: You are wrong, Beta. They *did* accept it, and their acceptance was a turning point in the history of mathematics. *This revolution in mathematical criticism changed the concept of mathematical truth, changed the standards of mathematical proof, changed the patterns of mathematical growth*![1] But now let us close our discussion for the time being: we shall discuss this new stage some other time.

Sigma: But then nothing is settled. We can't stop *now*.

Teacher: I sympathise. This latest stage will have important feed-backs to our discussion.[2] But a scientific inquiry 'begins and ends with problems'.[3] [*Leaves the classroom*].

Beta: But I had no problems at the beginning! And now I have nothing *but* problems!

(*Concluded*)

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[1] The concepts of criticism, counterexample, consequence, truth, and proof are inseparable; when they change, *the primary change occurs in the concept of criticism* and changes in the others follow.

[2] Cf. Lakatos [1962]

[3] Popper [1963*b*], p. 968

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ERRATA IN I. LAKATOS 'PROOFS AND REFUTATIONS' PARTS I-III

Part I

p. 2, last line:
Instead of ' Part III ' *read* ' Part IV '
p. 24, last line of the main text:
Instead of ' very small corner.' *read* ' very small corner.[2] '

Part II

p. 134, last line but one:
Instead of ' [1927] ' *read* ' [1925] '
p. 137, last line but one:
Instead of ' [1951] ' *read* ' [1941] '

Part III

p. 222, eighth line from the top:
Instead of ' V—E+F=0 ' *read* ' $V—E+F=1$ '
p. 225, third line of the footnote:
Instead of ' [1806-27] ' *read* ' [1826-27] '
p. 231, sixth line from the top:
Instead of ' suitable ' *read* ' suitably '
p. 231, first line of the footnotes:
Instead of ' p. 214 ' *read* ' p. 216 '
p. 232, second line of the footnotes:
Instead of ' [*c.* 195] ' *read* ' [*c.* 190] '
p.234, first two lines of the footnote:
Instead of ' Pascal [1654), 432 ' *read* ' Pascal [1654]. Cf. *Oeuvres complètes de Pascal, texte établi et annoté par Jacques Chevalier*, Paris, 1954, pp. 1206-7.'
p. 234, second line from the top:
Instead of ' criticism and logic?[1] ' *read* ' criticism and logic? '
p. 235, fourth line from the bottom:
Instead of " ' Certainty is never achieved ' " *read* " ' Certainty ' is never achieved "
p. 237, twenty-first line from the top:
Instead of ' Gamma ' *read* ' Omega '
p. 244, first line of the footnotes:
Instead of ' [1900] ' *read* ' [1899] '
p. 244, third footnote, eighteenth line from the bottom:
Instead of ' [1751]) ' *read* ' ([1858]) '
p. 245, tenth line from the top:
Instead of ' Beta's ' *read* ' Zeta's '

NOTE AND COMMENT

An Analytic Justification of Induction

ARTHUR Pap's analytic justification of induction seems to avoid the problem rather than solve it.[1] There is no difficulty in justifying particular inductive inferences; we do so by appealing to the rule of *critical enumerative induction.* Pap states this rule as follows:

> R: If you have reason to believe . . . that a large and varied sample of A's are B's infer that probably all or almost all A's are B's.

The problem, as Pap clearly saw, is how to justify R.

One could justify R by reference to a rule R′—Rules that have been successful in the past are to be used now—of 'higher type' in some (non-specified) theory of types. Although this avoids '*formal* circularity,' it leads to an infinite regress, which Pap found to be objectionable. A similar infinite regress is found, as Lewis Carroll observed, in the attempt to justify the following rule:

> P: Whatever proposition is implied by a true proposition is itself true.

Pap argued on the basis of the similarity of the *problems* of justifying these rules, that the solutions were similar. Let us examine the argument.

> To accept P can only mean to use the concept of 'implication' that P defines . . . If anyone had a reason for doubting P, then he would have to be able to specify the meaning of 'implies' . . . in such a way that it remains an open question whether P is true after this meaning has been specified. But this cannot be done because the relevant meaning of 'implies' is just defined by P. Similarly, it cannot be reasonable to doubt the validity of R unless the meaning of 'probable' can be specified independently of R. But is not R just as analytic of the meaning of 'if . . ., then probably . . .' as P is analytic of the meaning of 'if . . ., then . . . [. . . implies . . .]?'

Thus, Pap concludes

> . . . a statement of the form 'Probably E will happen' *means* that the present conditions are in the relevant aspects like conditions that in the past have usually been followed by E . . .

One could find several immediate problems in this argument. For example, one could point out that this analysis of 'probably' sheds no light on the probability of universal *theoretical* statements, statements with no observable 'instances'. There is, however, a major, and I think fatal, objection to this theory.

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[1] Arthur Pap, *An Introduction to the Philosophy of Science*, Free Press, New York, 1962. The relevant material is in Ch. 13, especially sect. G, pp. 238–44.

If 'Probably E will happen' *means* the present situation is similar to past situations in which E has usually occurred, what reason have we for expecting this present situation to continue to resemble the past ones? Although 'Probably E will happen' looks like it has reference to the future, it in fact refers only to the present and past.

Let us suppose the present situation S is similar to situations $S_1, \ldots, S_n$ in that conditions A, B, C, D obtain in S and similar conditions had obtained in $S_1, \ldots, S_n$. Suppose also that effect E had occurred in these *n* previous situations. Finally, suppose A, B, C, D are the only necessary conditions for E, and they are known to be necessary. (We are not supposing that A, B, C, D are known *to be all* the necessary conditions.) The inductive inference results in the belief that S will continue to resemble the $S_1, \ldots, S_n$ with respect to E. To claim that 'Probably E will happen' means 'S resembles $S_1, \ldots, S_n$ with respect to conditions A, B, C, D' does not even meet this problem.

The plausibility of Pap's argument rests on the ambiguity of the phrase 'relevant condition.' In the analysis above, A, B, C, D were assumed to be relevant in the only non-trivial and meaningful sense possible. If a defender of Pap's theory were to claim that A, B, C, D are relevant conditions if they comprise a *sufficient* condition for the occurrence of E, then he would claim too much. For, clearly, the occurrence of a sufficient condition for E warrants the inference to 'E will happen', not merely to 'Probably E will happen'. The phrase 'relevant condition' can be taken only in the sense of necessary condition.

That people (including scientists) expect S to continue to resemble $S_1, \ldots, S_n$ with respect to E is a fact which has not yet been justified. Perhaps facts ought only to be *explained*, not justified.

MORTON L. SCHAGRIN

DISCUSSIONS

Two Evolutionary Theories, by M. Grene; A Further Discussion

I write this note as a biologist, primarily a neo-biologist, and not a philosopher. I shall discuss the two opposing theories that Dr Grene considers[1] almost wholly as scientific theories and not as exercises in metaphysics. They *are* undoubtedly theories in science, and it seems to me that their value as such should be established before their metaphysical implications are considered. I have read the discussion by Messrs Bock and von Wahlert[2] and agree with almost everything they say, but I wish to add some remarks in analysis of the reasons that have led the great majority of biologists to prefer a neo-Darwinian[3] theory such as Simpson's to theories of the type of that put forward by Schindewolf.

To make my position clear, I must first give my views of how the theories of science should be related to each other. Clearly, every theory must be concordant with all the relevant facts, and must take account of the whole range of fact within its scope. Further than this, it must be concordant with any accepted theories that deal with narrower ranges of fact within the range with which it deals. So, we often have hierarchies of theories dealing with wider and wider ranges of fact. A theory of the history of the universe must be concordant with the accepted theory of the history of the solar system, that with those of the history of the world, of life on the world and of the evolution of animals as a part of that life.

But the theory of animal evolution is not that of lowest grade in this hierarchy. Biologists have long thought it undeniable that animals have lived throughout their evolution lives similar in general to those we find them living today in parts of the world undisturbed by man. It is in the succession of such lives in geological time that evolution takes place. Evolution is in fact a phenomenon in natural history, and our theories of evolution must be concordant with all our knowledge of the natural lives of animals.

To understand animal life in nature we need the results of all branches of biology—of genetics, and of the changes in morphology and physiology that genetical change can bring about, of animal populations and their reactions to the environmental conditions, and much else. All these ranges of fact are relevant to a theory of evolution, and all theories concerned with them will be subordinate in the hierarchy to the general theory of animal evolution. If a theory of evolution is to be acceptable, it *must* take account of all these facts and theories, and its value will largely depend on how far it does so.

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[1] This *Journal*, 1958, **9**, 110–27 and 185–93

[2] I have to thank the editor for the opportunity of reading their note before publication.

[3] I use the term 'neo-Darwinian' rather than 'synthetic', because the latter term seems to me vague and not discriminative. All theories of evolution are synthetic.

The neo-Darwinian theory in the form developed by Simpson passes this test. He does take account of all the relevant branches of biology and so builds his theory as to be concordant with the accepted results of them all. On the other hand, Schindewolf bases his theory almost wholly on what he can observe in his fossils and largely neglects other parts of biology. For instance, as Bock and von Wahlert point out, he neglects the modern work on the ecology of populations, treating evolution as change in the individual animal and disregarding the fact—which must be of great importance for any theory of evolution—that in bisexual animals living in interbreeding populations it is the population that evolves, not the animals within it individually. I believe one of the chief reasons why most biologists have not accepted his theory is this narrowness of his biological background.

Partly as a result of this narrowness of outlook, there is more than one point in Schindewolf's theory in which his views and assumptions are contrary to our present knowledge. I will discuss only one of these, but it is one that is fundamental to his theory, and indeed its main premiss. This is his belief that the characters of a new group of animals—its *bauplan* or ' type '—arise suddenly as a unitary whole and not gradually as is postulated by the neo-Darwinian theory. As Bock and von Wahlert point out, this is very difficult to believe unless we are ready to say that our present ideas of the genetic mechanism are entirely wrong. But, this is not the only objection to acceptance of his belief. It is founded on the observation that a gradual origin of a new group is rarely to be observed in the fossils; the animals of a new group usually appear suddenly. This is certainly true, but Simpson[1] has shown why it must be so if the Darwinian theory is accepted. He shows that even though the origin were gradual we ought not to expect often to find intermediate forms among the fossils. The large changes from one ' type ' to another must be expected most frequently to take place where a relatively small population of the first type is isolated in a restricted area in which conditions are different from those in other parts of the habitat of the species, for there the differences in the conditions will lead to rapid change under selection, and the small population and its isolation will favour the spread of any change. Also, since the chance of any individual being fossilised is notoriously slight, we are never likely to find intermediate forms for they will be few in number. It is only when a type has become established and successful—and has therefore spread over widely distributed environments—that we are likely to find it among our fossils. The suddenness of the appearance of new types is an inevitable consequence of the conditions of fossilisation.

Besides all this, it must be said that intermediates, although they are not common among fossils, are sometimes found. Occasionally the origin of a new type did not take place in a restricted area; we have then direct evidence that the origin was gradual. The evolution of the mammals from the reptiles is the example of the origin of a new type for which we have the best fossil evidence, and there we have very many intermediates. The characteristics in which the mammals differ from the reptiles appear one after the other in the group of reptiles from which the mammals arose, and in many cases the origin of each characteristic can be seen to be gradual. Indeed, the evidence is so complete that it is generally held that it is impossible to draw any but an artificial line between the forms that should be called mammals and those that are still reptiles. If this is so in the example for which we have the best evidence, it seems most improbable that the lack of intermediates in other examples is due to a very different process of evolution and not to the imperfection of the fossil record.

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[1] G. G. Simpson, *Tempo and Mode in Evolution*, New York, 1944, pp. 107-124

These arguments have seemed to most biologists to remove any need for the assumption of large and sudden changes in the origin of new animal types. But this, if accepted, destroys the main premiss of Schindewolf's theory.

For these reasons, as it seems to me, Schindewolf's theory is discredited but, besides her advocacy of his theory, Dr Grene, often following Schindewolf, puts forward several arguments that seem to her to raise difficulties for the neo-Darwinian theory. None of these will be accepted as difficulties by neo-Darwinians, but they need to be answered. One is that many organs could not in their small beginnings serve the purposes that the fully-developed organs serve, and therefore, it is said, they could not have been evolved gradually under selection. It is true that the earliest forms of many organs cannot have the functions of the fully-developed organs. Horns, for example, first arise in mammals, both in the life-history and in the evolution of fossil series, as small swellings on the frontal bones on the brow, and could not be useful for defence. But it must be remembered that functions as well as structure evolve, that the function of an organ often changes as the animal evolves. We see this very clearly if we compare the functions of the same organ in related animals. The small rudiment of an organ may be useless for the fully-developed organ's function but it is not necessarily valueless to the animal. In mammals the rudiment of a horn may be valuable in strengthening the skull in the region of the frontal bone and may be evolved for this purpose until it becomes large enough to be used for defence. We cannot conclude that the fact that a small rudiment would be useless for the present function of an organ makes the organ's gradual evolution from such a rudiment impossible.

Another argument that is accepted by Dr Grene as raising difficulties for the neo-Darwinian theory is that animals adapted to similar conditions and ways of life do not show similar structure in all their characters. Why, she asks (p. 191), if they are adapted to the same conditions are not their characters identical ? Since they are not so, ' there must be some structural, some " type " reference implied along with the adaptive one.' Undoubtedly there is; whales are not like fish in all their characters because they were evolved from a mammalian stock and retain mammalian structure, though greatly modified. Evolution is a progress in history, and it is accepted by all neo-Darwinians that the characters of an animal are determined in part—and in large part—by its history in evolution. To suggest that Simpson forgets this will be surprising to most readers of his books, though he certainly does not accept all the implications behind Schindewolf's use of the term ' type '. Bock and von Wahlert emphasise these implications.

Finally, I must mention one other criticism of the neo-Darwinian theory raised by Dr Grene. This is that the theory neglects the morphological aspects of evolutionary theory. She lays emphasis on this. I am not sure that I understand what she means here. If she means that neo-Darwinians neglect the evolution of the morphological characters of the body, that is demonstrably untrue. Adaptive characters are both morphological and physiological, and success in the struggle for existence depends equally on both. Certainly, the morphological characters are not neglected by neo-Darwinians in their discussions of adaptation and therefore of evolution.

She also says (p. 124) that neo-Darwinians do not give morphological characters priority and regards this as a criticism. It is true that they do not, but that is because they regard evolution as a process of change in the whole biology of the animal, and they therefore hold that in the interpretation of evolution no characters, morphological, physiological, behavioural or any other, should have priority.

In ways such as these it seems to most biologists that the objections to the neo-Darwinian theory raised by Schindewolf and those that think with him receive reasonable answer. Of the few that hold other views by no means all would agree with Schindewolf; Cannon was a Lamarkian and I doubt whether Waddington would accept Schindewolf's theory. But it must be said that neo-Darwinians do not hold that their theory gives a complete interpretation; no-one, I think, would claim that. Our knowledge of how the pattern of the body is developed during the life-history and changed in evolution, is very incomplete; the origin of mind is a very clear example of evolutionary fact beyond present explanation, and so is the origin of life for many biologists. I agree that the difference between neo-Darwinians and those who disagree with them is largely a difference of outlook, in the way they look at the problems of evolution. What neo-Darwinians would say is that their theory has a broader basis in biology, explains a much larger range of fact than any rival theory, and raises fewer difficulties than theories of the type of that put forward by Schindewolf.

This, even if it is accepted, is not enough to show that the neo-Darwinian theory is sound. It must also be shown that it is soundly based on principles that scientists can accept. In science the simplest theory that covers the facts with the minimum of *ad hoc* assumptions and without being contradicted by any facts, must always be accepted; it is in this sense that Occam's razor is applicable. In biology it is very clear that the living body is in large part controlled by the simple physical laws, though it is equally clear that the phenomena of life cannot all be described at present in terms of those laws, and perhaps they never will be. Still, for biology a materialistic explanation must always be the simplest, and we must therefore start with materialistic explanations and carry them as far as they will go, making assumptions that cannot be expressed in materialistic terms only when we are forced to do so.

The neo-Darwinian theory passes this test well, better than any rival theory. Accepting, besides the physical background, only the assumptions basic to all biology —those behind genetics, our interpretation of ontogeny, and so on—it covers a very large range of evolutionary fact in terms of our knowledge of the physical and chemical environments of the world and our general knowledge of biology. New observations are almost always found to receive reasonable interpretation in terms of the theory without *ad hoc* assumptions. This cannot be said to be true to the same extent of rival theories.

There is yet another test that the neo-Darwinian theory has stood well. This is the test of the progress of investigation. The history of genetics in the present century gives a good example of this. When Mendel's work was rediscovered in 1900, the large and sudden changes produced by the mutations he worked with seemed to be an impossible basis on which selection could produce the gradual changes that Darwin's theory required. Many biologists came to the conclusion that the theory had been falsified and must be discarded. So the position remained for thirty years, but in the 1930's and later a deeper knowledge of genetics has shown that just such gradual and continuous changes *are* produced in natural populations by numerous small mutations and complex interactions of the genes. As the result, opinion has swung back to belief in the theory. Today, almost all biologists accept its general truth in a form surprisingly little altered from that proposed by Darwin.

Finally, I must protest against Dr Grene's last paragraph (p. 193). I have discussed the theories throughout this note, as I said I would at the beginning, as scientific theories, but I must add that it seems to me that the neo-Darwinian theory must be accepted not only in science but also in metaphysics. A system of metaphysics interprets the nature of all reality. Its range is broader than that of science, and it stands at the apex of a hierarchy such as that described at the beginning of this note. On this hierarchy the scientific theories occupy lower positions. Where in metaphysics we deal with the results of science, it seems to me that we must accept the scientific theories that are most soundly based on scientific principles, and are not free to choose the theories that suit our broader metaphysical ideas best. I should say that the metaphysician is so free only if he denies the validity of scientific method as a whole. If he is not ready to do this, I think he must base his ideas on the interpretation that is most acceptable in science.

G. S. CARTER

TWO EVOLUTIONARY THEORIES: REPLY TO DR CARTER

THE main lines of Dr Carter's criticisms are met in my reply to my previous critics, but I should like to comment on some of the points he makes.

First, I must insist again that my article was not an essay in metaphysics but in methodology or epistemology. I was asking, from where we stand in the evolutionary process, what concepts we need for its interpretation. I was examining, in the first place, two scientific theories which 'look' at the same data from different points of view, and in conclusion I suggested that one of these theories admits concepts which the other has to pretend to do without, but introduces surreptitiously. The two concepts in question are ' spontaneity ' and ' type '. The use of neither concept entails indulgence in ' vitalism ' or ' Naturphilosophie ', let alone a denial of validity to ' scientific method as a whole '. Dr Carter himself has cautiously admitted them into his own book under the guise : ' the origin of some types of new pattern '.[1] Dr Carter wishes to refer me once more to the use of Occam's razor; I can only reiterate the complementary principle which I suggested in my article. No one wants to make ' *ad hoc* assumptions ' or to multiply entities unnecessarily ; but neither ought one to reduce concepts *beyond what is honestly possible.* The way in which neo-Darwinian theory does this, and the pseudo-substitutions in which it consequently indulges, I have analysed in my later article ' Statistics and Selection '[2]. No *philosopher,* I may say, would be allowed the accordion-like use of concepts in which, under the banner of 'scientific method ', neo-Darwinian evolutionists are allowed to indulge. I do, in fact, also agree with Professor Waddington that a metaphysic based wholly on the particulate entities which alone neo-Darwinism sanctions would seem to contradict the existence of consciousness and therefore to deny the existence of any theories whatsoever, along with the theorists who might hold them. This conclusion was clearly argued and courageously maintained by Dr Lack in his little book on *Evolutionary Theory and Christian Belief.* These metaphysical questions, however, were *not* at issue in my paper, although I have touched on them elsewhere.

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[1] G. S. Carter, *Animal Evolution,* Sigwick and Jackson, 1960, cf. pp. 4-12ff. 91, p. 345

[2] This *Journal,* 1961, 12, 25-42

Having misunderstood the general purpose of my paper, Dr Carter has also misunderstood some particular points, both in my text and in Schindewolf's theory. First, I do not believe for a moment that Schindewolf thinks ' individuals ' evolve rather than populations. True, Schindewolf is, as I pointed out, more interested in the differences between specimens of different periods than in speculations about the relatively continuous changes that may have produced them; in this emphasis he is just as one-sided on his side as Simpson is on his.

Secondly, the 'saltations' Schindewolf insists on are, on his own admission, probably the result of mutations early in the development of those individuals who are found to exhibit them: but this is just to say, as any neo-Darwinian might, that micromutations early in development may produce very large effects. There is here no difference in the facts read by one palaeontologist as saltative and by another as part of ' quantum evolution '.

Third, on the question of pre-adaptation, neo-Darwinians *can* of course extrapolate from known cases, like electric organs, to all of evolution. They *must* do so, in fact, if they accept as a first principle the premiss that organisms are adaptation-machines. This is a premiss, however, which makes nonsense of any attempt to distinguish ' higher ' and ' lower ' forms in evolution.

Fourth, Dr Carter has wholly misunderstood my argument about Simpson's derivation of the concept of ' adaptive type '. In fact, after looking up my article again, I still cannot see how he thinks that his rendering of what I am supposed to have said on p. 191 is related to what I did say there. I suggest that he consult in detail the argument of Simpson I am there analysing, together with my analysis of it. (Nor, again, are the ' implications ' of Schindewolf's type concept *implications*, but unfortunate associations in the minds of Bock and von Wahlert.) I know that neo-Darwinians like Mayr and Simpson are now eager to admit great hierarchies of concepts into biology; but the muddle in which Simpson still finds himself with respect to the concept of type is plain from his recent book on taxonomy, and, for example, from the analysis of this work by W. R. Thompson.[1]

Fifth, Dr Carter has also failed to understand my analysis of the relative priority of morphology and phylogeny in the two theories I was analysing. The statement he refers to on p. 124 was a comparison, not a criticism. Nor have I said that Simpson ' neglects morphology,' but that in his own terms he cannot admit its existence (see also the article of Thompson just referred to). One cannot in fact discuss the genesis of a pattern unless one admits that it exists; and the entities which neo-Darwinians, in their passion for economy, allow to exist, do not include structured wholes but only the statistical relations of the genetic materials constituting the gene pools of populations. The issue here is that raised in the article by Dalcq to which I referred on p. 188.

Finally, in answer to Dr Carter's allegation that I wish to defy ' scientific method ' —whatever that may be—I may say that I do not see why one must bow to the majority of biologists, as one is constantly being admonished to do, so long as that majority are demonstrably muddled and inconsistent. I prefer to follow the lead of a more critical, and, if I were given to the use of that popular epithet, more 'scientific' minority. Dr van Valen kindly referred me to a recent article by Mayr in *Science* and the correspondence resulting from it. May I in turn refer Dr Carter to the letter[1] by Dr S. Fiala included there. I am not suggesting that he will be convinced by it, any more than Professor Mayr was; but it is a plain statement by one more scientist, not a philosopher, who appreciates and regrets the dogmatism of the current orthodoxy in evolutionary theory.

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[1] *Studia Entomologica*, 1962, 5, 549–70

MARJORIE GRENE

EACH AND ALL

THE article by R. J. Diamond on 'Each and All'[2] hinges on a misstatement on page 283. T. Skolem did not prove that the numbers between 0 and 1 are enumerable. He produced a new proof of a result originally due to Löwenheim, according to which, if an axiom system is 'fulfillable', it is countably fulfillable. An axiom system is (countably) fulfillable if a (countable) set of individuals exists, and a set of functions defined on them, such that the axioms are satisfied.

In relation to the real numbers between 0 and 1, this result, and Cantor's non-enumerability proof, imply that if we have a set of axioms purporting to describe these real numbers there will, associated with these axioms, be a set of formalisable methods of enumeration. The formalised version of Cantor's argument will imply that any set S of individuals satisfying our axioms cannot be enumerated by *these* methods of enumeration. The possibility is not excluded, however, that a method of enumeration might exist, not formalisable by means of our axioms, which would serve to enumerate S. Unless we count Richard's paradox, Löwenheim's theorem is the first of the long line of incompleteness theorems that are now such an important part of mathematical logic.

The distinction which Dr Diamond draws between each and all appears to be closely related to the distinction involved in the concept of ω-consistency used by Gödel in his famous incompleteness proof of 1931. It is easy to show that we can have a formal system in which the proposition $P(n)$ can be derived for each $n = 0, 1, 2$ and so on, and yet the proposition 'for all n, $P(n)$ is true' is not derivable. This may be seen trivially from the fact that one could have a formal system in which each of the first set of propositions could be stated while the second proposition would not be formalisable at all.

All these points are discussed with great precision and care in Hilbert and Bernays's *Grundlagen aer Mathematik* (Berlin, 1934 and 1939). It is most unfortunate that the absence of an English translation of this model of lucidity should permit the perpetuation of misconceptions such as those of Dr Diamond.

G. A. BARNARD

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[1] *Science*, 1962, 135, 974-6

[2] This *Journal*, 1963, 13, 278-86

REVIEWS

KEY PROBLEMS OF SOCIOLOGY

Two of the key problems of sociological theory are do we need theories in sociology, and, if we do, how can we verify or test them?

Mr Rex[1] cites three activities which are often said to vindicate sociology's claim to be a science. These are classification, the search for laws, and the search for causes. He accepts all three as parts of sociology but carefully argues that none can proceed independently of theoretical advance. He argues that a theory of society is required even to define the subject matter of sociology. But there is no such theory. Yet empiricism (the doctrine that no theory is required) is unsatisfactory because either it confines itself to facts which are not necessarily sociological facts, or it treats social relations and institutions as though they were directly observable entities. Rex holds that such a metaphysics is repugnant to most scientists who prefer to regard institutions as theoretical models to explain and predict human behaviour. Rex then distinguishes

> observed behaviour, the theoretical interpretations which observed individuals put on each other's behaviour (we might call these 'participant's theories'), and the theoretical interpretation made by a sociologist of the behaviour of those he observes. If we do not make this distinction, we are in danger of treating as 'fact' entities whose existence has by no means been proved (p. 37).

The way to escape this danger, Rex argues, is to differentiate between social and other determinants of human behaviour, then to show what sort of statement would count as an empirical verifier of a sociological statement.

This raises the question of delimiting the subject matter of sociology and deciding to what extent its statements are empirically verifiable. Sweeping aside organicism and definitions of society, Mr Rex plumps for 'social relations', by which he understands the ends or purposes of the actor, his expectations, his knowledge of the norms and ends of the other actors, and the desire of the other actors to gain social approval. Theoretical models to explain human action utilising this idea of social relations are Mr Rex's idea of what sociology should aim at.

One objection to his view, that it is individualistic and more psychological than sociological, he answers by showing how his models take into account an actor's social relations to other people, and the influence of institutional and group norms, and are thus sociological. Another objection cuts deeper.

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[1] John Rex, *Key Problems of Sociological Theory*, Routledge and Kegan Paul, London, 1961, pp. ix+194, 25s.

This is that the models are 'subjective', that is they begin with an individual, his ends, and his knowledge, etc. Set over against this stands functionalism which concentrates on the objective needs of structures and societies. Mr Rex therefore attacks functionalism for its many false claims, but he concedes that in the notions of manifest and latent functions, it draws attention to the important difference between the immediate and isolated consequences of an act and the long term and systematic consequences. If I may interpolate an example: if I assassinate the dictator the manifest function of what I have done is to put an end to his life and his tyranny; in the long run, however, a civil war might break out and in the end everything might get much worse. A more orthodox example might be that in getting married I please myself and my family, whose continuance is ensured. In the long term I have helped social stability by binding together two family groups, thus strengthening the internal ties of the society.

After accepting this much from functionalism Mr Rex goes on to discuss whether subjectivity can have a legitimate place in an empirical sociology. He believes it can within his action theory, which cannot escape some subjectivity. The degree to which a person's ends, knowledge, and actions can be empirically tested varies. Rex's view is that there is a shading over from purely rational actions seeking well-understood ends, based on full scientific knowledge of the situation and logical deductions from these premises, through to irrational actions where the end may be vague or merely expressionistic, where there may be no understanding of the situation but merely arbitrary ritual practices to be undertaken.

Rex's model of society is that there are actors with conflicting ends, and that they tend to form themselves into groups with conflicting ends. The outcome of these conflicts turns on who has the power. Not only in politics, but in education, religion, and so on. Rex carries his model over into the analysis of social class and tries to show its superiority over both the Marxist and the empiricist analyses. His view is that there are groups in conflict, e.g. unions and employers, and that each develops an ideology, the superior group to legitimise its superiority, the inferior one to undermine the superiority of the superior group. This sort of sociology can be objective if it is explicit about its own values, and if it takes account of the balance of power—which can be objectively determined. It can be empirical science if it tests its attribution of ends, knowledge and norms to the actors.

> The position appears to be one in which sociology starts off as an interpretative science making ex post-facto analyses and devising plausible models to explain the facts which are observed. It cannot hope to demonstrate the validity of these models with the precision of natural science, but in the course of arguments proofs which rest on the logic of empirical science are introduced. Such proofs will consist of pointing to facts not previously considered in the original situation which would fit in with predictions in terms of the model and of rough comparisons with systems explicable in terms of similar models (p. 172).

Although I accept much of what Mr Rex says, there are a few points I want to develop in criticism of him, or rather of his model. The points I shall take up are his discussion of rational action, of the problem of the subjectivity of his model, of the aims of groups, of the importance of the balance of power, and of groups legitimating themselves with ideology.

Rex argues, I believe rightly, that in explaining human action we should begin with a model which assumes it to be rational, and then try to make adjustments to the model so that our deductions from it approximate more and more closely to what really happened. Now 'rational' means something rather complicated to Rex. Not only does it mean an action taken in pursuit of a clear end, it also must be an action logically deducible from empirical scientific knowledge. Rex does discuss actions based on what he calls 'ritual rules', by which I suppose he means magical and religious beliefs, but he seems reluctant to allow these as much rationality as those based on empirical science. This elevation of western empirical science into part of a canon of rational behaviour strikes me as a mistake; a mistake connected with his use of the expression 'ritual rules'. What is important is not the ritual rules of magic or relation, but the theories about the empirical world which are incorporated into magical and religious systems. It seems to me that a man in the New Guinea Highlands who acts to achieve his end (a jeep, say, or rain) by carrying out certain ceremonies, is acting as rationally, *within his situation*, as I would be were I to save money or drop dry ice in the clouds to obtain the same things. What is done is rational because it is based on the best available knowledge. The point is important because Mr Rex's view would imply that anyone who believes in and thus acts on anything which deviates from empirical science is less rational than one who does. This would mean that mariners who use Newton's refuted laws, would thereby lose some rationality, whereas in fact their using Newtonian tables is far more rational than using Einsteinian tables because with less trouble in calculation the former will get them from A to B. Moreover Rex's view would be impossible historically: since the behaviour of historical persons would at best be based on the out of date science of their time it would at best be not quite rational. Perhaps Rex would say that he meant only that behaviour is rational if it is based on the empirical science available *at the time*. But then this would allow certain systems of magic and religion to back up rational action. Besides, neither the world nor science is homogeneous; *what* empirical science is, at any one time, is not all that clear, and its availability everywhere is also not often the case. Is the New Guinea man any less rational when he acts than is the scientist in the backwoods university who has not yet received his copy of the latest scientific journal and who therefore acts on out-of-date theories?

Connected with rationality is Mr Rex's admission that his model of action admits of a certain subjectivity. I do not see much point in stressing this. A man's aims, knowledge, beliefs, and the unintended consequences of what he does all seem to me not to be irremediably private and subjective. The individualism of Weber and Rex strikes me as being as empirical and as objective as one could wish. A man's beliefs and his values are, so to speak, objective parts of his situation both to us and to him. If a man is a believing Catholic, or magician, for example, his actions may be radically affected, yet still rational. The only rationality Rex allows him is if his actions unintentionally promote some functionally desirable end. Yet it seems to me that a man's Catholicism is an objectively ascertainable fact about him, and that he is still capable of acting rationally to gain ends prescribed by his beliefs (like sending his children to what he knows are second rate schools, because he values the Catholicity of the schools more than their quality), or to avoid means proscribed by his beliefs (like contraceptives).

My final points are about Mr Rex's treatment of groups. First of all let me say that while I accept that we can ascribe aims to groups I do not see that groups can have aims. It is not clear what Rex believes, on this, but he goes on to talk of groups promoting ideologies which legitimate their standing. This he uses to construct a very ingenious theory of social class which says that much confusion is caused by the conflicting 'participants' theories' about social class. At times the way he talks drifts towards the idea that the groups cause their members to promote the group's interest. Such an idea is unnecessary because we can refer to the well-known disposition of many people to feel that the exising state of affairs is the only right one and who are thus prepared to defend it. And even given that the ascribed (manifest) aims of groups conflict, and given that they tend to develop rival ideologies, is power such an important and overriding consideration in society? It seems to me that naked confrontations of power are very rare in society. Conflict there certainly is, but surely it is normally resolved by manoeuvre, compromise, withdrawal, moral pressures and so on. The actual balance of power between groups strikes me as coming into play far less frequently than Mr Rex suggests it does. The use of economic and political power to resolve a conflict with victory for one side or the other is rather rare. For example, even in the House of Commons a censure debate is a naked confrontation of voting strength, but a controversial bill, with both parties in conflict is not always so decided on. Often the more powerful group will accept criticism, modify its proposals, and so on. This for reasons of rationality and common sense, not power.

This is a very well-organised and carefully argued book. But even though I happen to sympathise with its point of view, I found it somewhat abstract and heavy going at times. Far more of what was said could have been illustrated with concrete examples, perhaps a very small number, because the book has one thesis and every chapter is relevant to it and carries its development a step further.

I. C. Jarvie

The Foundations of Science and the Concepts of Psychology and Psycho-Analysis, Minnesota Studies in the Philosophy of Science, Vol. 1, edited by H. Feigl and M. Scriven.
University of Minnesota Press, Minneapolis; Oxford University Press, 1956. Pp. xiv + 346. 40s.

THIS is the first volume of Minnesota Studies in the Philosophy of Science, the production of the Minnesota Center. It consists of twelve papers ranging from a thoughtful 10-page note by M. Scriven on 'A possible distinction between traditional scientific disciplines and the study of human behaviour' to a 90-page essay by W. Sellars on 'Empiricism and the philosophy of mind', a valuable critique of sense-datum theories. Six of the papers are the individual or joint productions of the members of the staff of the center (H. Feigl, M. Scriven, P. E. Meehl, W. Sellars), five by external contributors (R. Carnap, B. F. Skinner, A. Ellis, A. Flew, and R. C. Buck), and one jointly by a staff member and an external contributor (Meehl and L. J. Cronbach). At the end of each paper is a bibliography, and the book begins with a Preface and then a valuable Synopsis of all the papers (pp. ix-xi) as well as the Contents. There is an adequate index.

As the Preface states, the studies of the Center from 1953 had 'been devoted largely . . . to the philosophical, logical, and methodological problems of psychology. Some papers in the present volume are concerned with the broader philosophical foundations, others with more specific problems of method or interpretation.' The papers were all (except for Skinner's) profoundly influenced by discussions held at the Center and the book is impressive for its variety as well as for its *unanimitas* (Skinner excepted). It is impossible in the compass of one review to deal fully with all the papers, but the book provokes:

1. some reflections on the functions of the philosophy of science;
2. an enquiry into how well these functions are here performed for psycho-analysis.

1. Philosophers of science in relation to the sciences seem to be in a position of parents to a family of precocious children:

(*a*) *They attempt to understand them*, both out of their own interest and for the sakes of the individual offspring, who in the last resort demand from the outside world support, attention, and approval for their products. Feigl and Carnap have clearly undertaken this task for several of the traditional sciences, and Carnap devotes the last six pages of his article (IX) to discussing the status of psychological terms in the light of the distinction carefully made in the main body of his paper between 'pure disposition terms', operationally definable, and 'theoretical terms', introduced by postulates and not completely interpreted.

Scriven has given deep attention to the activities and writings of B. F. Skinner, and in his article here patiently separates wheat from chaff; he obviously does it from enjoyment, but also for the sake of Skinner and his school (though Skinner's own paper suggests that as far as theory is concerned he is too much of a mixed-up kid to profit from this degree of understanding).

(*b*) *They attempt to keep order* among the various members and to prevent them discrediting one another. In his carefully worked-out analysis of one of Skinner's papers, Scriven devastatingly exposes the fallacies of an eminent scientist who has not stuck to his box but has tried to use his own success to forbid others not merely their different techniques but the same right to operate with ' open texture ' concepts and metaphorical language that he, seemingly unawares, arrogates to himself: Skinner is hoist with his own petard more than once. This is the sort of work that needs doing again and again.

Flew, in a beautifully constructed paper, is partly concerned that psycho-analysis shall not be discredited or discredit itself through provoking judgment by standards inapplicable to it.

(*c*) *They try to save them from wasting their efforts.* In particular they call attention to the empirical meaningfulness or otherwise of scientific concepts, and offer criteria of meaning. Feigl and Carnap are here concerned with this logical activity as applied to scientific theorising generally. Meehl shows how much of the activity of clinical psychologists may be wasted in composing profiles and could be better devoted to therapy or research.

The theories and concepts of psycho-analysis badly need clarifying, and Flew attempts such clarification for what he considers two of Freud's revolutionary concepts: the concept of unconscious motivation and the concept of motivation of involuntary behaviour. He indicates other tasks for the philosophy of psycho-analysis.

(*d*) *They may suggest things and ways to do them,* particularly ways of validating their theories, and they call attention to the problems involved in so doing. Several of the papers here are concerned with the complex relationship in any science between its Language of Observation and its Language of Theory; Feigl, Carnap, Scriven, and Sellars all have interesting and important things to say on this problem, and in their different ways are all agreed that the sharp distinction made between the two languages of a given science cannot be maintained. Sciences like psychology and psycho-analysis raise the problem of the distinction in an acute form. Sellars, in the last third of his long paper, develops an amusing ' myth to kill the Myth of the Given '. It is only too easy for psycho-analysts as well as philosophers to forget how much ' taking ' is concealed by the phrases ' clinical data ' or ' perpetual data '. In fact it might be argued that the emphasis on *data* has been unconsciously motivated by guilt over *capta, furta, rapta, laesa, et deleta* (see Sellars, p. 328).

(*e*) *They keep the family together* by keeping the different sciences in touch with the others' concepts. Sellars is well aware of these functions of the philosophy of science and brings out particularly strikingly how scientific theorising is not something radically different from ordinary thinking (' science is continuous with common sense ') but develops out of it; model-finding is as inveterate in thinking as it is crucial to the methodology of sciences. To be put in touch with each other's model-finding and -using is stimulating, and seems to result in the attempt to use the successful models of other sciences to advance one's own. For example, the findings of ethologists are now being ransacked by some psycho-analytical writers.

2. In these papers the examination of the Foundations of Science is undertaken in order to concentrate upon one question: whether psychology and psycho-analysis are bastard or legitimate members of the family. Thanks to Meehl, Cronbach, and Sellars the Concepts of Psychology come off rather better than the Concepts of Psycho-analysis, with the important exceptions of Flew's paper and the last half of Scriven's rebuttal of Skinner. Ellis's paper, ' An operational reformulation of some of the basic principles of Psycho-analysis ', starts well with a description of operationism and a statement of the need to reformulate psycho-analytic concepts, but the vocabulary he offers is inadequate and the reformulation does not seem to have been done with careful thought. For example, Freud's own forceful statement of the Oedipus complex is replaced by this:

> ' When . . . a boy learns that his father assumes an all-important position in regard to his mother, he frequently wishes he could usurp this position for himself. When, in addition, a boy's sex drives are denied easy outlet, . . . he often becomes sexually obsessed and intensely desires to have sex-relations with a great many women, including his own mother. As a result of his non-sexual and sexual jealousy of his father . . . a boy in our society may develop an incest complex and may desire to kill his father and/or fear that his father is going to castrate him. . . .'

How weak the ambiguity of that ' frequently ' and how misleading the suggestion that incest fantasies are the consequence of a pathological state called ' sexual obsession '. Put against the words of a (non-' obsessed ') boy of three and a half to his father as they played together: ' Then [sc. ' when I'm grown up '] there'll be two mans in this house; *and we'll have to send one of them away* '. Ellis's own reformulations read rather as if he were concerned to protect psycho-analysis against a possible charge of its being an un-American activity.

The extent of the acquaintance with psycho-analytic writing shown by the authors of these papers is extremely limited. In a bibliography of fifty-three items Ellis lists Freud's *Outline*, a paper by Rapaport, two books and two papers of his own, *and that is all.* Scriven evinces knowledge of what psycho-analysts do (e.g. on p. 115 he seems aware of hard work involved in the analysis of transference) but lists none of their writings; still, his chosen task is pulling Skinner to pieces. Flew lists four of Freud's books and two writers on the fringe of psycho-analysis; he also refers to the 'recent work of orthodox Freudians'.

Interesting as Flew's paper is, he confines himself to one aspect of Freud's own theories, namely to a part of what in psycho-analytic terminology is called, perhaps not very happily, 'instinct theory'; Flew thinks the five notions, *desire*, *wish*, *motive*, *purpose*, and *intention* are central to psycho-analysis and suggests (*a*) the elucidation of these notions as a task for the philosophy of psycho-analysis, and (*b*) comparison with other disciplines, especially history, concerned with men and motives. I should have been glad if he had taken up the clinical example which he quotes from Freud of a woman trying in a repeated sequence of compulsive behaviour to do (symbolically) two contradictory things, to hide and to reveal, and had then gone on to a philosophical examination of one or two concepts of 'object-relations theory', in particular *identification*.

Sellar's paper in sections IX to XVI does seem to provide an epistemological foundation for the 'inner world' language of modern psycho-analytic 'object relations' theory. He claims to show that 'the fact that overt behaviour *is* evidence for these episodes' (sc. of thought, etc.) '*is built into the very logic of these concepts.* Indeed, introspection constitutes a use of these concepts which *is built on and presupposes* their intersubjective status.' In other words, an object-relationship is the presupposition of inner experience.

This book is one for philosophers; that it is not one for psycho-analysts too is partly due to the failure of psycho-analysts to make themselves understood or to expect understanding, but partly also to the failure of the philosophers to understand or to expect to be able to. Which is, after all, the usual story of a broken relationship between offspring and parent. The publication of this book marks the *intention* to try to mend it.

John Padel

An Introduction to Wittgenstein's Tractatus. By G. E. M. Anscombe.
Hutchinson, London, 1959. Pp. 173. 12s. 6d.

Of the three books that have been published in recent years in English on Wittgenstein's *Tractatus Logico-Philosophicus*, Miss Anscombe's is both the most interesting and the most difficult. And it is these two things for the same reason: because it presents a thorough analysis of the early Wittgenstein's main ideas as well as of the most elliptic and therefore apparently obscure passages in the *Tractatus*. Miss Anscombe attacks and deals with all those points that one might like to skip over in reading because of their formal difficulty or their inconsistency with other parts of the book. And she succeeds in interpreting every one of them in such a way that in the end one does understand them—or at least thinks one does. I personally must confess that I had not really understood how the formula $[\bar{p}, \bar{\xi}, N(\bar{\xi})]$ would indeed yield all the possible values of a proposition, until I had read her explication of it.

The one thing, however, that the book is not, is an 'introduction'. It assumes throughout a considerable familiarity with the thoughts in the *Tractatus* and knowledge of Russell's main arguments. I even found that lack of a thorough acquaintance with Frege's writings would at times be an obstacle to the understanding of Miss Anscombe's discussions. When, therefore, I said that her book was 'most interesting', I did not mean that it was easy reading or that it presented a well-paved road to the understanding of the *Tractatus*. On the contrary, I found parts of it more difficult than the latter itself. The excuse that Miss Anscombe can claim for this, is that her book is the result of comprehensive research on Wittgenstein's Manuscripts—and also on the works of Frege—and that it presents a frightfully clever piece of original thinking on the logic in the *Tractatus*.

For it is the logic behind Wittgenstein's conceptions, particularly of the Picture Theory of Propositions, and their analysis into truth-functions, that the book is mainly about; and this is perhaps also its main shortcoming: it says hardly anything about the world that language is said to be mirroring, but deals exclusively with the mirror. Of her thirteen chapters, only two concern the informal treatment in the latter part of the *Tractatus* (concerning Solipsism, Knowledge, Ethics, and 'Mysticism'). All the others are an analysis of the logic of propositions (elementary and others), of operations, negation, generality, formal series, and symbolisation. It will now be necessary to discuss some of her arguments in detail.

I

At the opening of her book, Miss Anscombe deals with what she believes to be one of the most widely held misconceptions about the nature of the elementary propositions, namely that they are observation (or even sense-datum) statements. She gives two reasons why this view must be wrong: (*a*) elementary propositions cannot contradict each other (4.211)[1] whereas observation statements easily can; and (*b*) because 'there is hardly any epistemology in the *Tractatus*' (p. 27), since Wittgenstein believed that

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[1] Numbers with decimals refer to the statements in the *Tractatus*, page numbers are those of Miss Anscombe's book.

'psychology is no nearer related to philosophy than any other natural science' (4.1121). This, Miss Anscombe thinks, 'is fantastically untrue' (p. 28). She says that Wittgenstein held five theses about elementary propositions:

(1) They are mutually independent.
(2) They are essentially positive.
(3) They can be true or false only in one way.
(4) There is for them no distinction between internal and external negation.
(5) They are concatenations of names which are absolutely simple signs.

She gives an analysis of each of these points, and a particularly illuminating example for (3), to show how only a *complex* proposition could be false in more than one way. Namely, if either the complex 'thing' mentioned in the proposition did not exist, or if what was predicated of it did not apply to it.

Regarding the argument why internal and external negation must be the same for elementary propositions, she says that while two contradictory statements must not have anything in common (5.143), the sentences 'the King of France is bald' and 'the King of France is not bald' do have something in common, namely 'the King of France (exists)', and can therefore not be contradictions, which are always cases of external negation. She then shows that there can be only *one* proposition that lies entirely outside a given elementary proposition (namely its contradictory). And since all elementary propositions must be independent of one another, it follows that they can have only one kind of negation.

In a certain way, Miss Anscombe's entire book is concerned with Wittgenstein's Picture Theory of Elementary Propositions, and I should therefore like to quote what she herself says about it.

> It seems sure that the *Tractatus* account [of the picture theory] is wrong. This is partly because one cannot believe in the simple objects required by the theory; partly because it leads to dogmatic and plainly false conclusions about the will, about modality and about generalizations in infinite cases. But it is a powerful and beautiful theory: and there is surely something right about it—if one could dispense with 'simples' and draw the limits of its applicability (p. 77).

And regarding her treatment of it in her book, she says a little earlier on (p. 72), that,

> there are indeed serious difficulties about it; nevertheless, we shall understand the *Tractatus* best if we let ourselves succumb to the attractiveness of this idea, assume its correctness, and follow up its consequences throughout the *Tractatus*.

One of the consequences of the picture theory was, she believes, that there can be only one kind of necessity in the world, namely logical necessity. And she says that Wittgenstein held this view *ad hoc*, for the sake of consistency. I feel quite sure, however, that consistency was not Wittgenstein's main reason for his disdain of the 'causal nexus'. For whether a possible state of affairs does or does not hold in a certain part of space-time, is for him entirely a matter of contingency. The logical structure of the world once given, whatever relation between actual things materialises is completely accidental, and this contingency is part of his conception of the world as 'what is the case'; it constitutes the play that logical necessity leaves for the configuration of things—and his picture theory of language, in which sentences mirror this logical structure of the world, certainly does not prejudge the issue. The inference (or mirroring) is done the other way round from the way Miss Anscombe in many places of her book seems to imply.

2

Miss Anscombe has taken very seriously what Wittgenstein said in the Preface to the *Tractatus*, viz. that 'I owe in large measure the stimulation of my thoughts' to Frege and Russell. Four of her chapters deal with the distinctions between the related views of Russell, Frege, and Wittgenstein on the sense (meaning) and reference of names and propositions, and on objects and functions. She says, correctly, that for Wittgenstein names had no sense, only reference, but incorrectly (I believe) that 'a name has reference only in the context of a proposition' (p. 102). If she means to include proper names among these (as I think she must), this is surely wrong. I should have thought that names are the *only* words that do have reference outside of a context, and I feel sure that Wittgenstein would have thought so too. Similarly I cannot agree with her that 'what is a function *of* something is not a function *tout court*' (p. 103). But what is 'a function *tout court*'? Is it not one of the essential characteristics of a function to be *of* something? She makes this remark *à propos* Frege's idea of presenting a predicate in a sentence as a function f, whose variable x is the subject of the (grammatical) sentence.

Neither can I quite agree with her discussion of Frege's distinction between an assumption (Annahme) and an assertion (Behauptung). The assertion-sign $\vdash$ that he introduced and that Russell adopted in *Principia Mathematica* says that the proposition which follows this sign *is true*, whereas the unasserted proposition (which Frege prefixed by $—p$) corresponds to the assumption that p (is the case), while 'p' alone represents the thought (Gedanke) expressed by p. Now Miss Anscombe says that she can see no difference between p and $—p$, since 'a "thought" is not a proposition, not even an unasserted proposition, but is the *sense* of a proposition, and hence there is the same *Gedanke* when we have a proposition and when we have one with the stroke attached' (p. 106). She rejects Frege's, and Wittgenstein's, idea that an *Annahme* contains a hypothetical assumption and thinks that Wittgenstein falsely speaks of 'the Fregian *Annahme*' as if *Annahme* 'had been a technical term in Frege' (p. 105). But of course it *is* a technical mathematical term which does postulate the provisionary assumption of the proposition as true. (E.g. every *reductio ad absurdum* proof in mathematics begins with the words: 'Angenommen dass . . . '.) And so Wittgenstein was quite right to attribute to Frege this interpretation of the stroke — which he then rejects for formal reasons as an illegitimate introduction of a psychological belief of the speaker into a purely logical argument. Frege's reason for the introduction of the assertion sign was that a distinction seemed necessary between the occurrence of 'p' in 'if p, then q' and in '$p \vee q$' on the one hand, and the plain assertion that 'p' ('is the case'). In other words, it is the difference between entertaining p as an hypothesis and asserting it as true. And while of course the 'p' is the same in both cases (the *Gedanke*, as Frege would say), the way it is given to us is different. I am not so sure that this difference is only a psychological one, as Miss Anscombe believes, and not also a logical one. Russell has spoken of the 'non-psychological sense' of being asserted, but she says that '"assertion" has *only* a psychological sense', for 'it can be no part of the requirements for [a proposition's] being "logically asserted" that . . . [it] be true' (p. 116). But can't it?

The reason why I should nevertheless agree with Wittgenstein that in a treatise of logic the assertion stroke is misplaced, is that one should not introduce a symbol of self-reference into a proposition of logic.

We meet with a somewhat similar kind of argument in the first of her two chapters on 'Negation', where she discusses the question whether the *truth* of 'p' is the same as the truth of '$p . (p \supset q) \supset q$' and answers that *intuitively* it is *not* the same. What she does not say is that if p is true, it is so by correspondence with a fact, whereas the truth of $p . (p \supset q) \supset q$ is guaranteed by the consistency of logic, and is therefore 'truth by convention'; and this amounts to saying that the one is a synthetic, the other an analytic statement. This question arises with her introduction of the truth-tables and the assignment of truth-values (*a*) arbitrarily to the bases (atomic propositions), and (*b*) of the resulting T- or F-values to the respective truth functions (molecular propositions). These chapters are perhaps the only introductory ones in the book, in so far as they explain, and do not assume prior knowledge of, the meaning and use of the truth operations.

3

Another one of the consequences of the picture theory is that when everything has been said with propositions that state how things are, there is something left over that cannot be said, although it is part of the 'real' (i.e. observable) world: namely the logical form, or structure, that sentences must have in common with the situations they describe, *if* they are to be able to be descriptions at all. One of the relations that cannot be described but must be 'seen' (or that 'shows itself through its symbol') is the membership of certain things to the class of *formal concepts*. Wittgenstein names a few of these concepts, and Miss Anscombe adds several. They are e.g. number, function, object (Gegenstand), fact, complex, predicate, proposition, relation, etc. This implies that one cannot meaningfully say 'there are objects', nor 'there are 100 objects' (4.1272), just as it would be, even intuitively, meaningless to say 'there is a 4' or 'there are sixes'. Wittgenstein, Miss Anscombe says, gives 'a wholly new account of the formal concept of number' (p. 124) through the use he makes of the method of *iteration* in a formal series, which consists in the repetition of an operation. The notation he uses for this is:

$$\Omega^{0\prime}x,\ \Omega^{\nu\prime}x,\ \Omega^{\nu+1\prime}x\,, \qquad (6.02)$$

where he defines: $x \underset{\mathrm{df}}{=} \Omega^{0}$

and $\Omega'\ \Omega^{\nu\prime}x \underset{\mathrm{df}}{=} \Omega^{\nu+1\prime}x,$

so that the number becomes the exponent of an operation.

While I found Miss Anscombe's discussion of this point unenlightening, the following chapter on 'the General Form of Propositions' contains what I find to be a most excellent explication of Wittgenstein's very cryptic treatment of the 'general term of a truth-function', viz.

$$[\bar{p},\ \bar{\xi},\ N(\bar{\xi})]. \qquad (6)$$

While Wittgenstein tells us only that 'this says nothing else than that every proposition is the result of the operation $N'(\bar{\xi})$ to the elementary propositions', Miss Anscombe actually shows us how in the case of two elementary propositions p and q their 16 truth functions can be generated by this formula (pp. 132 ff). Unfortunately she does not also give the mathematical proof of why the method she suggests will yield only previous results after all the truth-functions have been derived; nor does she tell us how, in a long series, we are to recognise that a term has occurred before, although such a proof can, of course, be given.

Nevertheless, her treatment of this formula is most illuminating and clear. In the end, she suggests that Wittgenstein's account seems plausible for finite series only, but that he did not think there was an essential difference between the finite and the infinite case, thereby leaving us with one of the main problems of the *Tractatus* still on our hands: its inability to explain generality in an infinite world.

4

At the end of her book, Miss Anscombe discusses in non-technical language two major points regarding the interpretation of the *Tractatus* by later philosophers. She asks, firstly, how it was possible that the *Tractatus* could have had such a profound influence on the Logical Positivists whose cardinal interest was epistemology, whose paramount method verification, and whose 'Sesame' the sense-datum, when there was nothing about either, theory of knowledge in general, or sense-perception in particular, in the *Tractatus* at all. Her answer is that Schlick took Wittgenstein's theory of sentence-meaning (namely that the meaning lies in its truth-conditions) to imply that we are asking for a description when we ask for the meaning of a statement. And this description, according to Schlick, consisted largely, or at least finally, in ostensive definitions. This, she thinks, is how the verification theory came about. She believes that this development is the result of a misinterpretation of Wittgenstein's intention, since it is quite a different thing to assign a reference to the words of a sentence (as the *Tractatus* requires) than to show what situation it describes or what observations would verify it. For Wittgenstein, an elementary proposition describes a situation because it has the same internal structure or ' form ' as the atomic fact, but this is a formal requirement and not an empirical one.

Her second point concerns Wittgenstein's ' mysticism '. She believes that the word has a very special meaning for him which has nothing to do with the usual connotation (such as ' super-natural ' or ' extra-natural ' explanations). For Wittgenstein, only that can be *said*, of which there is a negative, or at least the possibility of one; all the rest can only be shown. Regarding metaphysical statements she says (p. 163): ' If you want to argue that something is a philosophical illusion, you cannot treat it as a false hypothesis. What you can infer from it is not a contradiction, but an incoherence.' This, I think, is absolutely true and, by the way, explains a good deal of our helplessness *vis à vis* muddled philosophical arguments or ' systems '. She is less right, I think, when she counts Wittgenstein's Solipsism among the unsayable, the Mystical. If solipsism cannot be convincingly, or even properly, expounded, the reason is not, I believe, because it is something that shows itself in the structure of things or their symbolisation, but because it cannot be communicated, and therefore not phrased in a proposition of language, since it excludes the possibility of a hearer. If there were such a thing as a private language solipsism could be stated in it. She thinks, incidentally, that Wittgenstein did believe in the possibility of a private language, at least at the time of writing the *Tractatus*. I feel quite sure that he did not, and find this borne out by precisely his treatment of solipsism. Solipsism, in its extreme and therefore only logical form, has something of the circularity of self-implication about it, and this differs widely from the requirements of the ' unsayables '.

Miss Anscombe finds support for Wittgenstein's belief in a private language in the much-debated sentence (5.62): ' That the world is *my* world shows in that the limits of that language (*the* language that I understand) means the limits of my world.' The bracketed phrase (which in German reads: ' die sprache, die allein ich verstehe ') she has taken to mean ' the language that I alone understand '; but this is false. It means: ' the *only* language that I can understand.' And this, I believe, can be interpreted, quite to the contrary, as the idea that language, being the mirror of the world, is common to us all, our only means of communication, and at the same time, the only way of expressing out thoughts, i.e. our understanding of the world. And while to each of us the extension of the mirror (its limits) may differ somewhat, and while the pictures it reflects appear from different angles, according to our point-of-view, it has parts which can be seen and investigated by everyone (the world of discourse) and boundaries beyond which nothing is reflected (beyond which lies the unsayable). In this way, language becomes the only intelligible thing for us in the world, and we have to turn to it if we want to understand anything about the latter—which would constitute an excellent reason for Wittgenstein to have written the *Tractatus* and for Miss Anscombe to analyse it the way she did.

EVA CASSIRER

The Existence of Mind. By John Beloff.
MacGibbon and Kee, London, 1962, pp. 263. 35s.

MR Beloff is a psychologist worried by an issue which he feels has for too long been swept underneath the philosophers' carpet: this is the issue of the relationship between mind and matter. He refuses to be put off with what he regards as wily linguistic ruses or the ritual chanting of fashionable philosophical mottoes, insisting that: 'What matters most is to return an *honest* answer, even if one knows it can be no more than provisional, and never to pretend that an issue has been closed when, in fact, it is still patently wide open.' His book is indeed honest, as also well-written, and we should be grateful to him for his attempt to grapple with problems that must surely perplex all but the philosophical ostrich.

Mr Beloff does not offer any new arguments on the mind-body problem in general, but prefers to attack the problem via more detailed discussions of Behaviourism, Perception, Thought, Action, Personal Identity and the Paranormal. Even so, he does not suggest any radically new solutions, concentrating rather on attacking various positions held by many psychologists and philosophers today. Both Watsonian and Ryleian types of behaviourism, he says, are conceptually confused and unable to cope with certain empirical phenomena—including those of parapsychology—which it would be dishonest to ignore. Even 'automatism', i.e. the doctrine that there is no essential difference between human thinking and machine-computing, has to be rejected—though its subtlety and power may initially seem very persuasive. We cannot deny the existence of mental states, and must choose between some sort of 'double-aspect' theory and some form of dualism. Mr Beloff opts for the latter, stressing a causal as well as an ontological dualism (the brain—a natural computer—is the *instrument* of the mind), though he does not suggest how we may overcome the old arguments against interactionism. He also opts for a Representative Theory of Perception, a transcendental theory of the self, and a belief in freewill, in each case making good points against opposing views but again failing to argue convincingly for his own positions. Consequently it is usually unclear what, exactly, his position is, and one can neither accept nor definitely reject his conclusions.

One confusion in particular leads to unclarities in the chapters on Thought and Action, where he discusses the relationship of consciousness to the higher mental processes. There are two broad questions which Mr Beloff raises, and which he uses as crucial weapons in his polemic, but he fails to distinguish them clearly from one another. They are : Is consciousness an idle epiphenomenon, or does it have some functional rôle to play (either dynamic or directional) in cognitive processes? Secondly, are conscious states fully determined, and if so are physico-chemical laws sufficient to account for them?

Even self-modifying, heuristically flexible computers, says Mr Beloff, will always be crucially different from human thinkers, in that—although they are symbol-manipulating machines—they can never use these symbols to assert a proposition. The thorny problem of what it is to 'assert a proposition' is discussed very briefly in terms of a concept of 'intentionality' which seems to come down to consciousness. It is assumed that no machine can ever possess consciousness, and it is also assumed that a deterministic approach will always deny function to consciousness, ignoring it as an irrelevant evolutionary excrescence. Mr Beloff does not mention those psychologists—such as Bartlett and the post-Vigotsky Russian school—who have suggested a very real and crucial rôle for conscious imagery, both in adult life and in the child before efficient symbolic forms of representation are available to him. Although he discusses the notion of 'emergence' with some sympathy, rejecting reductionist accounts of science, Mr Beloff implies that to explain states of consciousness in terms of the laws of neurophysiology or communication-engineering would be to deny the power of consciousness to influence or to effect any changes, even in other conscious states. How then could he acknowledge the solvent effect of compounds on other compounds, none of whose constituent elements are soluble? Perhaps he would say, in line with his general dualist position, that consciousness is so different from chemistry that the two cannot be compared—but this way of talking would lead him into further difficulties, only too obvious ever since the time of the Occasionalists.

Similarly, Mr Beloff holds that freewill requires not only that conscious states be functional in directing the activity of the organism, but that they also be not determined. It is not enough that they be unpredictable or merely 'conditionally' predictable as on the MacKay model ; nor, I think, would he be satisfied with the reply that determinism, if true, is trivial, since we can never specify all the individual characteristics of an event before subsuming it under a system of general laws, which laws are themselves merely a convenient shorthand. Mr Beloff ends up with what he describes as a libertarian's compromise between determinism and indeterminism: choice-events are 'self-determined'. I do not understand what is meant by this, unless it is that conscious events may be influenced by other conscious events rather than being either random occurrences or fully explicable in terms of neurophysiology alone. But this is no solution: the question which Mr Beloff takes as crucial still remains. Do the neurophysiological and mental events together constitute one causally 'closed' system? Or must consciousness remain outside the causal nexus?—labelling it as 'self-determined' certainly suggests this. Since Mr Beloff would certainly not wish to equate this 'self-determination' with the 'undetermined' processes of stochastic computers sometimes used as models for trial-and-error learning, a fuller discussion of this concept would have been welcome.

The bibliography includes many useful references to recent works on psychology, parapsychology, and information-theory, as well as references to philosophical discussions of the problems raised by Mr Beloff. His book may not provide us with fresh answers, but it should goad us to fresh thought.

MARGARET A. BODEN

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